SCIENCE

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*THE LAST QUARTER—A REMINISCENCE AND AN OUTLOOK.**

NINETY years ago, a botanist holding a professor's chair in Williams College for the supposed mismanagement of an estate in Columbia county was confined for a short period in a debtor's prison in New York City. Years afterward he related to a friend that as a relief to the monotony of confinement he found amusement in teaching botany to the keeper's son whom he described as a bright youth of fourteen years. From such an inauspicious beginning came the real development of botany in this city, for while Hosack had attempted to develop his Elgin Gardens earlier in the century, the above episode was the beginning of a career that resulted in the rapid advance of botanical science in New York. It is only proper to add that the professor above noted was no less a personage than Amos Eaton, author of the first series of American botanical manuals, and the willing pupil was none other than John Torrey, the Nestor of American Botany.

Were we tracing the full pedigree of botany in New York, it would be necessary to follow the record two generations back of Torrey, for it was Hosack, the originator of the first botanic garden of New York who instructed and assisted Amos Eaton in his early botanical studies while the latter was still a law student in New York City, and more specially after he had passed on to his higher work of instruction. Hosack's Botanical Garden at 54th Street and Madison Ave. was too far out of town for the New Yorkers of 1801–1806 to visit, and it passed over finally to Columbia College and laid a solid foundation for the financial endowment of that institution, as property

* Address of Retiring President, Botanical Society of America.

advancement followed settlement northward up Manhattan Island. It was little wonder that this college early came to foster botanical science and later accumulated the foundations that have led up to its present tender of facilities for botanical research along varied lines.

It is unnecessary in this presence to relate in detail the incidents which led up to the development of a botanical center here as early as 1831, so that Asa Gray, restive in his work in Central New York and casting about for a place where he could study botany, could find no better tutelage than under his master Torrey, and came to New York as Torrey's pupil and finally became his assistant in the preparation of the Flora of North America, a work that will ever stand as a masterpiece in American botany, combining with the critical acumen and exact learning of its senior author the enthusiasm and push of its more youthful one. It may, however, be useful at this time to call to mind some of the conditions existing at the time of the first appearance of Torrey and Gray's Flora in 1838 or even at the period of the issue of the final part of the second uncompleted volume in 1843. The great Louisiana Purchase of 1803 extending northwestward from the mouth of the Mississippi to the Pacific had scarcely been entered by the scientific explorer except in its Northern portion, and that mainly by Lewis and Clark in their discovery of the headwaters of the Columbia and by Long's expedition to the Rocky Mountains. Texas and the great Southwest, Utah, Nevada, and California were quiet, Mexico-Spanish possessions alike undisturbed by the hum of civilization or the visitation of the field botanist except as some wandering explorer like Adelbert Chamisso had touched at the Pacific ports and had skimmed a few memorials of the vast west coast flora, or some Russian expedition had pushed down from their northern possessions into Northern California. Minnesota and the Northwest were still in the hands of the Indians, and all of Iowa and much of Illinois were raw prairie untouched by the plow of the pioneer. Chicago was a hamlet with a handful of people struggling with fever and ague on the wind swept marshes at the lower end of Lake Michigan. The South which even yet has scarcely produced an indigenous botanist was then a region untouched since the travels of Michaux, except as Short and Peter had explored Kentucky and Stephen Elliot, the father of Southern botany, had brought to notice something of the flora of the Carolinas. Such in brief was the state of our country and its botanical exploration when Gray received his call to Cambridge and laid there the foundation of a second center of botanical research. The annexation of Texas as the second of our Spanish acquisitions of territory; the Mexican war with the commencement of our expansion policy in the cession of California and New Mexico with the attendant military occupation and exploration for the settlement of boundaries; the discovery of gold in California in 1848 and the attendant development of that Eldorado of immigration, and finally with the transcontinental railroad projects of the early fifties, all brought to Torrey and Gray the floral wealth of these extensions of territory and have made the Torrey herbarium at New York and the Gray herbarium at Cambridge the two great repositories of the types of western plants, each supplementing the other in their priceless possessions.

Few of the present generation of botanical students realize clearly the rapid advance of their science in the past quarter of a century or the conditions under which the student of botany was placed at the beginning of that period. It is just an even twenty-five years since your retiring president completed the solitary course in bot-

any offered in his undergraduate collegiate work in 1875. It was a course of lectures given by a great and good man, but one whose first love was geology and not botany, and extended through a short term of twelve weeks in which we were instructed in some of the details of the structure of the flowering plants something after the pattern set in Gray's lessons, after which we were directed how to use Gray's Manual for determining the unknown names of such familiar plants as the Trillium, the spring beauty, the wild geranium and the white daisy with all the array of names like *Leucanthemum chrysanthemum* that nearly paralyzed such of the students as had pursued a long course in Greek. There was scarcely a word as to the homology of parts, relationship of plants to each other or to their environment. Not a word was breathed about the world of cryptogamic organisms; the ferns and fungi were alike tabooed, and liverworts and lichens may as well have had a non-existence for we never heard them mentioned, and went out of college ignorant of their existence at least from any direct information from the instructor. The compound microscope was sealed to us except as an illustration of the application of the principles of optics, but we well remember the half day of unalloyed pleasure when we stole into the room where it was usually securely locked in its case and for once found the case open. How we reveled in a set of prepared slides and had our first self-taught lesson in plant histology.

This personal reminiscence is not an unusual picture for those times, for then there were in the country only two or three colleges where there was a distinctive professor of botany, and even in these more favored institutions the character of the instruction was much the same as I have pictured. Ecology was unheard of in the schools; plant physiology was scarcely mentioned and indeed its only printed exponent available was 'How Plants Grow, Gray.' Evolution was some unholy doctrine about monkeys that contradicted the Bible. It was with the force of an electric shock that a short time later the translation of Sachs' Botany opened to our astounded eyes the manner in which we as students had been robbed of the knowledge of the splendid advance of the science that had been in progress in Germany during the middle half of the present century. Soon after this, Bessey's 'Botany' for schools appeared, and it is no exaggeration to say that since the time when Amos Eaton's first class in Williams College begged the privilege to publish for him the first and most famous edition of his manual, no single book has appeared that for its time has proved a more valuable contribution to botanical teaching in America. Bessey's work was particularly useful at this time because it served to introduce the younger student to Sachs' more extensive and difficult text-book and showed him that there were other and broader considerations in botany than the mere 'analysis of flowers,' and gave him for the first time a rational conception of that underworld of plant life of which the hitherto one-sided facilities for study had robbed him. Since that time wave after wave of lines of botanical investigation and methods of teaching have swept over us, and system after system of elementary instruction has been proposed and has been crystallized or more often presented in an amorphous condition in the numerous text-books and laboratory manuals of the past fifteen years.

I should add here that indirectly a second factor greatly stimulated the development of the new botany, namely the introduction of elementary biological study in the schools, for about this time Huxley's 'Biology' appeared and from an English stimulus American students commenced the development of biological investigation from a new stand-

point. Laboratory methods were commenced, and laboratory equipments followed. But Huxley was mainly a zoologist, and thus not unnaturally it came about that some American biologists came to be developed in a one-sided way, and in some cases came to assume the unfortunate proposition that biology was only another name for zoology. In later years they learned their mistake and for the future the student who hopes for success along biological lines recognizes that he is committing a fatal error if he does not prepare for his future with a vigorous foundation in plant biology.

Twenty-five years ago there was practically one American botanist and his manual was supposed to be the end of all necessary knowledge even though its descriptions often failed to cover variations noted that later botanists have dared to call species. In some remote quarters the momentous question was occasionally presented to the teacher of botany, Is Gray's system really better than Wood's? but usually there was little dissent from an affirmative answer to the question.

It was in the latter part of this same year (1875) that the first number of the *Botanical Bulletin* (now the *Botanical Gazette*) was issued by the enthusiastic professor of natural science of a little college in southern Indiana. It was a four-page sheet without cover containing mainly notes on the local flora of the vicinity of the college and bearing little prophecy of its present development into two volumes aggregating nine hundred pages a year. Five years before The Torrey Club of New York had founded its *Bulletin*, but for the five years prior to 1875 had not produced so many pages as have just been issued in the first five months of the year 1900. Something of the aim of the latter journal in its early days may be of interest at this time. We quote from the first number, January, 1870: "An attentive study of plants in their native haunts is essential to the advance of the science, and in this respect the local observer has an advantage over the explorer of extensive regions, or the possessor of a general herbarium. He can note the plant from its cradle to its grave; can watch its struggles for existence, its habits, its migrations, its variations; can study its atmospheric and entomological economies; can speculate on its relations to the past, or experiment on its utility to man. It would be in vain to attempt to enumerate all the points about which a lover of vegetable nature can learn and report something new. Botany, like every other science, far from being exhausted, is ever widening its field." This language for the time in which it was written was an unexpected prophecy, for at this period scarcely anyone looked at botany as a serious subject. It was regarded as a suitable study for misses' boarding schools and a harmless elective in a few of the more enlightened colleges. No facilities were open for advanced work and none was thought of by the college authorities. A botanist to grow must delve for himself, must invent his methods, must acquire unaided the mastery of his implements of research. If, as was usually the case, he was short of means, he must either teach or practice medicine and from his overcrowded hours steal the time in which to devote himself to the object of his love. With most of us it was a literal exemplification of Agassiz' rule that to succeed as a naturalist one must 'lie hard and live low,' and many in the struggle lost their ambition and went in pursuit of more lucrative subjects.

The government, except in now and then detailing a botanist and too often an untrained one on some exploring expedition, had not yet commenced to foster botanical research. There was a botanist, to be sure, to the Department of Agriculture and he did accumulate an extensive collection so long

stored in the old fire trap that, be it said to the disgrace of our Government, still serves to house this Department; and this collection, fortunate in escaping the hazards of fire, formed the nucleus of the splendid national herbarium now housed in the crowded attics of the National Museum.

Such is a faint picture of the conditions of American botany as they existed twenty-five years ago. What American botany has become to-day with its centers of scientific research scattered throughout the country from New England to the Golden Gate, with its botanical publications in a healthful and hopeful if not prosperous state of development, with its agriculture and the arts clearly recognized by the state and general governments, with the work of the experiment stations which in many states have already passed their formative stages and are entering the lists as centres of intelligent botanical research, in the forestry reserves which are already fields of practical experiment and promise also to form similar research centers, and finally in the attitude of men of thought and men of wealth who are devoting their activities of brain and purse toward better making known the importance of the plant world to man in its utility and beauty, and seeking for the advancement of botanical knowledge throughout the earth,—these are the products almost of the past two decades and measure a progress unparalleled in the history of scientific development in any country and in any age. And this progress has commenced along diverse and in some cases unexpected lines. After instruction in botany ceased to be merely the analysis of flowers, came the study of types leading the student to consider other subjects than the flowering side of plant creation opening up new lines in morphology and plant development; then came plant physiology taking inspiration from the labors of Sachs and others across the sea; then followed plant cytology from the enthusiasm kindled in the American students who for years have formed the greater number of workers in the famous laboratory at Bonn; later still plant embryology has for the time been the botanical fashion and has opened up new points of view for the study of plant relationship.

To revert once more to Amos Eaton who may be regarded not only as the pioneer of the long succession of botanical teachers in America, but as well the author of the first series of botanical manuals that have made a knowledge of our flora accessible to students, we desire to call attention to what he considered the cardinal preparation for the work of a botanical instructor "No one," said he, "should attempt to give instruction in botany who is not thoroughly familiar with at least four hundred plants." It has been reserved for the present generation to produce botanical teachers who do not know plants and those whose lack of botanical perspective becomes intensified in the successive crops of highly specialized students that come out from their instruction; and we may well emphasize this early principle which in the present generation is even more essential as the first equipment of a genuine botanist than it was in Eaton's time. This leads us naturally to some thoughts and propositions concerning the future of botany in America, and some lines of training necessary to make this future satisfactory. It will be readily admitted that aside from applied botany which has for its work the making known of the plant world for the greater comfort and enjoyment of the human race, one of the highest aims of purely scientific botanical study whether cytological, morphological, or embryological, is to determine the relationship of plants to each other and the establishment of a natural system of classification. This has been the struggle of botanists from the time of Jussieu to the present and every

genuine botanical contribution attacking the problem continually from a new standpoint adds its iota to this desired end. The undergraduate course of botany therefore to attain its best success either as a preparation for life or as a preparation for botanical or biological research should not commence the fad for specialization too early. It ought thoroughly to familiarize the student with the common types of plants not alone in the laboratory and in pickle but in their living freshness in the field. The broadest as well as the most intimate acquaintance with as wide an array as possible is none too much. Besides the four hundred plants of the higher type that Eaton recommended as a minimum, the student of to-day ought to know half as many at least from the array of lower plants which in Eaton's time were a world almost unknown. And all this should be in hand in addition to the best attainable methods of study with the means of using to the best advantage the modern implements of research from the simple lens and the dissecting needles which should not be neglected, to the microtome and the various culture media. Following this, the research course for those who really expect to be botanists, to attain its highest results ought to involve some phase of systematic botany, for otherwise the student confining himself to too narrow lines is still in danger of losing his perspective. I do not mean systematic botany in its old-fashioned narrow sense but in its newer and broader sense in which morphology, embryology, and the widest application of accessory study in culture house, laboratory and field is brought to bear on the solution of a taxonomic problem.

The line of research study that is primarily embryological ought not to be confined to a single type, but should involve the comparative study of the types of a family or in the case of restricted families the relations to outliers in others. If the major line of research involves the life history of a species among the lower plants, it ought also to involve a comparative study of the allied American species, for no one could be better prepared to monograph a genus than one who from his studies of development understands the significance of morphological and structural characters, their degree of variation, and their cause. We have had enough of these studies of single things without bearings or relations, made by candidates for a degree whose botanical work is sure to end with the theses that they have been able to produce with the bolstering aid of their instructor, studies that mean nothing because the workers failed to grasp the real purpose of the study and its true relations and bearing on the general subject. It is too late to accept candidates for degrees in botany, who seek the degree as an ignominious end rather than a well-earned beginning and preparation for future work along botanical lines. Whatever we may be obliged to do with and for those who take undergraduate work in botany because they have to, we have no time for graduate triflers or any who are not intent on making botany their life work—who study botany not because it is a present fad or because they really think they have to study something, but because they are botanists born and must become botanists trained. Such will find their thesis the beginning of a series of researches instead of the swan song of a neophyte. In order to limit a wide subject more closely and continue our consideration of fields opening up to the trained botanical student, we may turn in particular to the field of work that lies open to the systematic botanist in America. For the higher plants I can do no better than to quote a recent remark of my successor in office, whose personal studies give to his opinion far greater weight than any words of mine, that there is scarcely a large genus represented in

America that is not in real need of revision, and surely the unparalleled activity of recent workers in this field not alone in the west and south, where novelties were more naturally to be expected, but even in New England and other parts of the Northeast, would seem fully to justify the assertion. Intelligent field study of which we need much more by trained botanists rather than by untrained field collectors is yielding its natural and legitimate fruit. And long before our ecologists can attain definite point to their labors we must know with far greater precision than now the definite limitation of the species of our higher flora.

When we turn to the lower plants we find a still more virgin field in which to sound the call for able workers; here not only is the need of revision real, but it is crying and in some cases almost shrieking in its need. Among the fungi the desultory description of species 'supposed to be new' must give way to careful revision, genus by genus and family by family. Field work and culture methods must supplement the most searching comparative study of the widest accessible array of material, and while it will certainly result in great reductions of species carelessly described by the older methods and in the correction of many errors of over-hasty reference of American species to those of European origin, it will also result in bringing to light many that by the same methods have been entirely overlooked. We could at once place twenty men carefully trained for research at work on as many groups of fungi in which our definite knowledge is only a hazy mass of crude and scattered bits of information needing to be brought into harmony with the results of careful consecutive comparative study. And beyond matter of the definite limitation of the species within the genus and the genus within the family, there are beyond and more far reaching in their scope the most important questions of alliances and origin of groups that demand the highest efforts of the most careful workers.

Among the Algæ the conditions are still worse, particularly among many fresh water forms, for here not only are our species in a haze of uncertain definition, but the very characters to be used in specific definition in many groups still remain to be formulated.

To accomplish this systematic study of our flora successfully will require the active co-operation of field botanists and special students. No longer can one man or a few men seek to realize the complete definition of even the higher flora much less the lower. There must be the most complete and candid co-operation on every hand. To this end we offer a few suggestions.

1. More and more the real systematic study of our flora must proceed from the centers where are gathered the stores of material and among these centers there ought to be the freest possible interchange of courtesies. Isolated workers in the field whose training is such that their judgment regarding specific limitation is worthy of respect, before attempting to publish their results should supplement their field and home study by an occasional year or at the very least frequent long vacations at some one of the centers where contact with books and types and still more with other workers and methods would correct their results and stimulate to better effort.

2. More and more the local field workers and amateurs should realize that as isolated facts are worthless until brought into harmony with some general law, so material illustrating the distribution and variation of certain plants is only valuable when deposited where it is accessible to botanical workers generally. Great collections are only good as well as great when the material they contain is accessible to those who are trained to study it, and local col-

lectors ought to realize that by freely supplying the material within their reach they confer a favor not so much to the particular center that receives it as upon all future workers in that field who look to that center as the proper repository of the material they need to carry on their researches. The most obscure field collector by concentration of observation rather than by diffusion of effort may add material that will be of vital importance to a correct and complete knowledge of our flora. More and more these field workers must be content to wait for results and not be clamorous for their material to be named by the next mail, resting assured that the material they contribute will receive due recognition on the final results and that none of their work that is really useful will go to waste; on the other hand the specialist taking his material in order will gain time for his final revision that would otherwise be badly broken up in miscellaneous examinations and too often marred by hasty determinations and conclusions.

The different attitude of people toward the botanist and toward certain of the scientific workers in fields where there is a much more decided commercial phase to the science, resulting in part from the fact that real botanists are such because they are first lovers of their subject, is well illustrated in this matter of the freedom with which material is sent to botanists for examination and report. Fancy a man sending to a well known chemist a half dozen samples of water for chemical analysis with the assurance that if any of the water is not used up in the process, the chemist may have it for his trouble!! And yet no one thinks of anything else in sending a botanist from a half dozen to two hundred specimens in a bale with the information that he would like them named and the specimens need not be returned. To do this correctly involves skill that requires fully as great an amount of scientific preparation and often more expenditure of time than to make a similar number of water analyses, yet to the botanist the idea of pay has not been presented. While it is true that by this self-sacrifice many of our great collections have been enriched with valuable material, and many solitary workers have been encouraged to better things, it is also true that it has been accomplished with an immense waste of time and nervous energy that ought not to be too lightly demanded of active workers. And this labor is all the more exasperating when some botanical worker who does not appreciate the necessity of abundance of good material brings in from some distant region merely a series of odds and ends of fragments that are yet sufficient to show that something really worth having might have been collected by a little expenditure of common sense and foresight on the part of the collector.

3. There are some practices rife even among those from whom we naturally expect better things which ought in some way to be put under the ban. I need only mention the practice of literary revisions of genera with no commensurate study or even knowledge of the plants themselves; the so called galvanic method of transferring species heterogeneously from one generic group to another except in definite monographic work; and the description of species in check-lists, fly-leaves, circulars, and other out-of-the-way places where they are not readily accessible to the general botanist. In short the closing years of the nineteenth century ought to sound the death knell of the literary enthusiast and the galvanizer and mark the disappearance of the grotesque and the rafinesque from American botany.

4. The extent of territory covered by the American Flora is on the eve of a remarkable expansion. The political events of the past few years are in part responsible for

this, but for years it has been evident that it was only a question of a little time when such expressions as 'a revision of the species north of Mexico' should be obsolete in American botany. The extensive explorations of Mexico and Central America by Anglo-Saxon American explorers have resulted in placing the flora of those countries in the hands of American botanists, and the Spanish War has had among its other results the transfer of the flora of the Antilles to the same hands. And with the extensive explorations that in recent years have been made by our botanists in South America, I fail to read the signs of the times correctly if they do not point to the ultimate inclusion of all the American continent from Alaska to the Straits of Magellan in the future of American systematic botany. Scarcely anything can be expected of our neighbors in the Central and South American States and many Europeans overlooking American work already accomplished or wilfully ignorant of it, are too often unsatisfactory in their results; surely it is at once the privilege and the duty of the botanists of this country to take the initiative in a vastly more extended and systematically organized way than has ever been attempted and solve the most important problem that remains in the study of phyto-geography; the flora of the darkest of all dark continents—South America. Explorations on a stupendous scale, such as are necessary to attack this problem, can best be organized here in America where the accomplishment of great things is no longer a novelty. The English already realize that they have problems sufficient on their own hemisphere fully to engage their attention; and it will not be long before our German friends will realize the same situation. It is most natural that Americans who are more familiar with conditions here and are more at home with the flora of the upper portions of the American continent, should be the ones to undertake this work and it is most certain that they will carry it out with greater accuracy because of their ability and proximity. I think it behooves the Americans practically to withdraw from any except the necessary comparative study of the flora of any part of the Eastern Hemisphere, and combine with such division of labor as will be practical and equable on the study of the flora of the American continent. In this way by example at least we can show our friends across the sea that in botany at least the Monroe doctrine is still a living and practical issue. I might add that those who are hoping for an international congress originating in Europe to settle questions of nomenclature, or any other line of botanical policy or procedure, entirely misunderstand the tenor of European methods, or the genius of European conservatism and inertia; such as wait for a European congress to settle anything, will wait until their descendants of the third generation are tottering with age before such an adjustment will really occur. Americans must take the initiative in all these matters if they are ever to be settled, and the Old World must follow the lead of America in every field where progress is involved.

But all this extension of the study of our flora, even that which is limited to the North American continent, means years of study that cannot be accomplished on this side of the water. We are debtors to the Old World in that they found our continent and with that discovery took back to their own the memorials of their conquest. Not only are the great herbaria at Kew, at the British Museum, at Berlin, and at Paris the repositories of the types of many of the species of the American tropics but in these great collections are what are often more valuable than the types themselves, namely extensive suites of plants illustrating better than the type itself the range and varia-

tion and allies of the tropical species. To these greater collections we should add the lesser at Copenhagen, Geneva, Vienna, Prag, St. Petersburg and Madrid, all of which must be visited again and again before we can fully define even our North American tropical flora. To this end it will be in order to look to the establishment of traveling fellowships that will make possible continued study in those European storehouses by trained American specialists.

The time has come for American Botany to assert itself in the modest way that becomes Americans, and assume its true position in the work of botany in the world. We have the men who have profited from the training of the best the Old World could produce in morphological, physiological, and cytological work among plants; we have young men trained and in training who have the mental acumen of the best any country can produce, combined with a degree of practicality, vitality and energy of which very many of the Europeans are lacking; we have men of means who are philanthropic toward botanical research and stand willing to aid in every work that merits recognition, and if we in America do not in the next quarter of a century lead the world in matters botanical, it will be because we are not true to the instincts that guided the fathers in botany and because we do not enter into our heritage and magnify our opportunities.

Lucien M. Underwood.

Columbia University.

ARTIFICIAL PARTHENOGENESIS IN ANNELIDS (*Chætopterus*).

My experiments on the artificial parthenogenesis in sea-urchins have led me to the following results: (1) Through a certain increase in the osmotic pressure of the surrounding solution, the unfertilized eggs of some (probably all) Echinoderms can be caused to develop into normal blastulæ or plutei. (2) This increase in osmotic pressure can be produced by electrolytes as well as by non-conductors. It is therefore probable that the parthenogenetic development is caused by the egg losing a certain amount of water.*

I considered it necessary to try whether the same results can be obtained in other groups of animals by the same means. I have recently succeeded in producing artificial parthenogenesis not only in starfish (*Asterias*), but also in worms (*Chætopterus*). The experiments on the artificial parthenogenesis in Annelids led to the unexpected result, that the unfertilized eggs can be caused to develop into apparently normal larvæ (*Trochophores*) by two entirely different methods: First, by increasing the concentration of the surrounding solution (osmotic fertilization). This method is qualitatively the same as the one by which I produced plutei from the unfertilized eggs of Echinoderms. Second, by changing the constitution of the sea-water without raising its concentration (chemical fertilization). Through a slight increase in the amount of K-ions in the sea-water the eggs of *Chætopterus* can be caused not only to throw out the polar bodies as Mead had already observed, but also to reach the *Trochophore*-stage and swim about as actively as the larvæ originating from fertilized eggs. Further experiments showed that the K-ions have no such specific effect upon the unfertilized eggs of Echinoderms. This fact may help us to understand why a hybridization between worms and Echinoderms is impossible. I shall publish a full report of these experiments in one of the next numbers of *The American Journal of Physiology*.

Jacques Loeb.

Woods Holl, July 22, 1900.

* Loeb, J., *The American Journal of Physiology*, Vol. IV., August, 1900.

THE ASTRONOMICAL AND ASTROPHYSICAL SOCIETY OF AMERICA.

II.

Photographs of Nebulæ and Star Clusters obtained with the Crossley Reflector: By J. E. KEELER.

The photographs on glass were exhibited by Professor Campbell, who called attention to some of the difficulties which had to be overcome in the use of the instrument and to the important results established, that nebulæ as a rule are spiral in form and as an exception irregular in form.

Some New Spectroscopic Binaries: By W. W. CAMPBELL.

A list of half a dozen spectroscopic binary stars, recently discovered by Messrs. Campbell and Wright at the Lick Observatory. (To be published in the *Astrophysical Journal.*)

A Simple Method of inserting the Comparison Spectrum in Spectrographic Observations: By W. H. WRIGHT. Read by PROFESSOR CAMPBELL.

The length of the slit is limited by two total reflection prisms mounted immediately in front of the slit plate. The reflecting edges of the two prisms are parallel to each other and at right angles to the slit; and the distance between the reflecting edges is the length of the slit. The artificial light, from two sets of electrodes placed one near each end of the full-length slit, enters the slit through these prisms near their vertices. The method possesses many advantages. (To be published in the *Astrophysical Journal.*)

Color Screens as applied to Achromatic Telescopes: By GEORGE H. PETERS.

Attention was called to the want of definition caused by the secondary spectrum in large refractors. It was then shown how this was removed at the U. S. Naval Observatory by adapting a cell filled with an absorbing solution between the eye-piece and eye of the observer in the 26-inch telescope. The thickness of the absorbing layer is about 6 mm., and the solutions used are the following:

1. Picric Acid and Chloride of Copper dissolved either in water or alcohol, for cutting out the red and blue light.
2. Picric Acid dissolved in alcohol or water.
3. Bichromate of Potash in water.
4. Chromate of Potash in water.

Solutions 2, 3 and 4 remove the blue and violet light. Alcohol is used in cold weather. The absorption of the liquids is tested by a direct vision spectroscope.

Some of the benefits derived from the color screen are:

Improved definition on planets and satellites and in certain classes of double stars.

In observations of objects by day where the blue light from the sky is removed.

Professor See and Mr. Peters are jointly credited with this device. (See *Astronomische Nachrichten*, No. 3636.)

The Spectrographic Determination of the Motions of the Stars in the Line of Sight: By W. W. CAMPBELL.

Complying with the Council's request for a paper on this subject, the following points were presented:

The great interest taken in this problem is attested by the fact that the largest refracting telescopes in the United States, France, England, Germany, Russia and Africa are, or soon will be, largely devoted to the determination of stellar velocities.

It must not be assumed that, because a 36-inch objective receives nine times as much light as a 12-inch, it will be able to carry velocity determinations in that ratio to the fainter stars. The larger telescope is accompanied by stronger absorption of the light, by larger stellar images (except in perfect seeing), by greater difficulties in guiding, etc.; so that it is able to extend the

work of a 12-inch telescope only 5- to 7-fold. A 36-inch reflector, suitably mounted, and in a dry climate, would probably be more efficient than a 36-inch refractor in this problem.

Success in determining stellar velocities depends upon a great many elements, of which the following were all that time permitted a mention.

I. A suitable compromise between the various optical and mechanical features—many of which conflict—to secure a powerful and efficient spectrograph.

II. Suitable prisms and lenses. The requirements demanded by the line of sight tests are so severe that satisfactory lenses and prisms are difficult to obtain. Since a change to a new prism-train usually requires many changes in the mounting, the prisms should be constructed and thoroughly tested before the rest of the instrument is designed.

The day for using the ordinary double camera lens is past. Even the triple lenses do not fulfill the difficult spectrograph's requirements.

III. Absence of differential flexure during the exposure time: the optical angles must remain constant. This is very largely a problem in mechanical design. The elimination of appreciable flexure effects during long exposures is difficult, but not impossible. Existing spectrographs, including those just now completed, are wrongly designed, in that they are supported only at one end. The heavy prism-end projects out into space, unsupported. The spectrograph is held out 'at arm's length,' so to speak. Now the spectrograph is an instrument complete in itself, and need not be rigidly connected with the telescope tube. A framework connected with the telescope could be arranged to support the spectrograph, in position, both near its upper and its lower ends; care being taken that strains in the supporting framework should not be communicated to the spectrograph. This form of support would permi a more economical distribution of material in the spectrograph, and should decrease the flexure effects several fold.

IV. The Mill's spectrograph has been enclosed in a wooden box case, lined with thick hair felt, carrying on its interior surface about 30 feet of German silver wire. As the temperature within the case falls, a storage battery is connected with the resistance wire. The heat generated tends to keep the instrument, and especially the prisms, at a constant temperature—a most important desideratum.

V. An accurate method of guiding during the exposure.

VI. The use of the finer dark lines throughout the spectrum, rather than of the heavier lines which have heretofore been the ones measured.

VII. Long experience in making the measures.

An exposure of one hour is required for a star of the 5th photographic magnitude, as given in the Draper Catalogue.

At the Lick Observatory two or more plates have been secured for each of about 300 stars. For the best stars, such as *Polaris*, the probable error of a single determination of velocity is about one-third of a kilometer per second. For such stars the plates seem to be essentially perfect, and to secure greater accuracy it appears that a more powerful spectrograph is required.

Even with perfect plates the observer must be on his guard against systematic errors arising from changes in his personal habit of measurement.

Of the 300 stars observed, 22 have been found at the Lick Observatory to be binary, in addition to three previously detected from the same list by Belopolsky. This leads us to the important conclusion that one star in twelve is a spectroscopic binary. And it is probable that others of the stars observed are binaries as yet undetected.

Some Peculiarities in the Radial Velocities of ξ Geminorum: By W. W. CAMPBELL.

This is a well known variable star, period of $10\frac{1}{6}$ days. Its velocity in the line of sight was found, independently at Pulkowa and Mt. Hamilton, to be variable. Forty-five observations at Mt. Hamilton, distributed over $1\frac{1}{2}$ years, left no doubt that this star is a spectroscopic binary, and that the variation in brightness is produced by the companion. The eclipse theory is untenable. The variation is probably the result of tidal action between the two bodies.

The observed velocities cannot be represented by elliptic motion. Drawing the elliptic curve which best represents the observations, the observed curve is alternately above and below it, crossing it at intervals of about 41 hours. Three complete periods of the minor irregularities coincide with one light period. These minor irregularities in the observed velocities are probably due to minor tidal effects in the star's atmosphere. (To be published in the *Astrophysical Journal*).

H. M. Paul. I would like to ask if these observations were plotted to represent the total amount of observations, obtaining the period from these observations.

Mr. Campbell. They were all plotted with reference to the brightness of the star assuming the old period. There is some doubt as to the exact form of the light-curve. I have endeavored in every possible way to ascertain the form of the light-curve; but there seem to be some irregularities, and I think that extremely accurate observations would show that the brightness curve has the same irregularities as the velocity curve; that there is a reflex action of one upon the other.

G. W. Hough. Were there any systematic observations to ascertain the cause of irregularities with reference to motion in the line of sight?

Mr. Campbell. No; we have obtained our results merely as by-products from the system of guiding that we have employed; and we have made changes in the system of guiding in the expectation that if we could eliminate the difficulties arising from that, so as to make it satisfactory for one bright star, it would be satisfactory for two stars.

Mr. L. E. Jewell. Mr. Campbell has suggested that the irregularity in the motion in the line of sight may be due to tidal action, which I think is probably the case; but he has also suggested that it may be due to tidal compression, which I do not think can explain the phenomenon. I have myself been engaged in some work bearing upon that problem, and among other things the pressure of the different layers of the solar atmosphere; but the lines which have been measured in the solar atmosphere do not have a pressure of more than $1\frac{1}{2}$ atmospheres, considering the pressure of the earth's atmosphere as one. The variation due to difference of pressure would be too slight to produce the effect that he speaks of. I think it probable some tidal action causes the motion in the line of sight, and that it is not due to change of pressure, because the differences of pressure would be too slight to produce such an effect. In the case of the sodium lines in the sun's atmosphere, the actual pressure is only about an atmosphere and a quarter. The variation in the hydrogen lines would be very small, and it is probably produced in a higher layer of the sun's atmosphere.

Mr. Campbell. We seem to have no analogous case to this star from which to obtain data to explain it. We have no knowledge of tidal action elsewhere, but here we seem to have enormous evidence of tidal action.

T. J. J. See. I think the explanation of the cause of the second curve superposed upon the first curve is very probable. In the case of the ocean tides, we have very little vertical motion and a horizontal

motion many times greater. The distortion shown in the figure indicates that the horizontal motion is very large.

W. J. Humphreys. Some work has been done by Wilssing at the Potsdam observatory, showing a great change from change of pressure; but he applies this to such stars as Nova Aurigae. He has obtained results, working with hydrogen, showing that the spectrum lines in hydrogen are very sensitive to diminution of pressure. This applies especially to the deeper lines. He has obtained perfectly sharp lines with a pressure of 14 atmospheres.

Mr. Jewell. From our knowledge of the solar spectrum it is perfectly certain that the lines do not show any evidence of powerful pressure. Under such pressure the lines would probably be broader.

The Indian Eclipse Photographs of Jan. 22, 1898, obtained by the Lick Observatory Expedition: By W. W. Campbell.

Photographs of the corona and its spectrum; and of the spectrum of the sun's edge, were thrown on the screen. Special attention was called to the apparent and probable connection between the prominences and the curved coronal streamers enclosing them and to the relative displacement of the bright and dark lines in the spectrum of the sun's edge.

Total Solar Eclipse of May 28, 1900.

Simon Newcomb presented the report of the committee upon the observations of the recent eclipse.

W. W. Campbell. Being a long distance from our base in California, it was impossible to take our plates home for development, and they were developed in camp, where it was impossible to do photographic work successfully. I have brought one of the original negatives, but Mr. Brown should have the privilege of presenting it. It was taken with an exposure of one second, with a 40-foot camera. It reveals the fact that the coronal tails were not so numerous as those in 1898, while those were not so numerous as in 1893. There seems at first to be no connection between the coronal streamers and the prominences; and yet, if you will go over the plate carefully, I think you will see there is some connection between them. Our spectroscopic material was not so great as we should have liked to obtain; but for the third contact we obtained very satisfactory results. It was a question whether bright lines would be shifted towards the red end, but they seem to show no evidence of such shifting.

S. J. Brown. The eclipse observations of the Naval Observatory were widely distributed and covered a great deal of ground. Two main stations were occupied, one in North Carolina and the other in Georgia. Our photographic work is not in a condition to exhibit. There was one experimental line of research I think it worth while to mention. We wished to try experiments with color screens; and with one lens we used isochromatic plates, and with another lens the ordinary photographic plates, to see whether any indications would be shown as to a difference of structure of the corona due to the green line. The result showed no difference of structure, but there seems to have been a greater expansion of the polar rays when we used the color screens than when we used the ordinary plates. Photographs were taken to obtain as great an extension of the corona as possible; but the sky was not favorable, and we did not obtain so great an extension as was shown by the results in India. The spectroscopic work occupied our most serious attention. We had some very rare gratings, ruled to concentrate the light of the first order at a special point. Two of these gratings, from the Johns Hopkins University, were utilized, one at Pinehurst and one at Griffin, besides another grating of long focal length. Our preparations for this

work were much hurried, but we had gratings of which the curves were carefully computed for the focal length of the quartz lenses. At Griffin we obtained a reversing layer of eight seconds. Mr. Jewell has brought some copies of the only successful plates at Pinehurst, showing several new coronal lines, one coinciding with the 1474 lines, and there were three other new lines. Dr. Dorsey of the Johns Hopkins University also took photographs, and Dr. Wood of the University of Wisconsin undertook to study the shadow bands which precede and follow the eclipse. He is not here, but he thinks he has reached a satisfactory conclusion with regard to the cause of the shadow bands. While our results in spectroscopic work were disappointing, in view of the large outlay, they seem to vindicate the value of that method. We think we shall be able to obtain a plate which will bend to the required curvature, and thus be able to photograph the reversing layer.

L. E. Jewell. We obtained a different wave length for the bright and for the corresponding dark lines; and I think that difference in wave length is caused by the fact that the dark lines were produced very close to the moon's edge, by the remaining portion of the crescent, not covered by the moon, while the bright lines were produced further out. On one of the photographs taken at Pinehurst there is a very distinct difference of the *H* and *K* lines, between the bright and dark portions of the spectrum. The bright line is displaced with reference to the dark line in the direction outside, or further away, from the sun itself, just as it should be if that were the true explanation. In determining the method we should follow in the use of these plates we were largely influenced by the results of Evershed in India in 1898, showing that the chromosphere and reversing layer were much brighter in the upper part, showing other interesting phenomena. Another interesting thing was the presence of the rare element scandium. It was thought best to use his grating for the purpose of our work, and I made comparisons showing that he used a dispersion about three times that of a single prism; and we concluded that we could obtain at least as much light from our grating as from a prism. Our results at Pinehurst were thought of extreme importance. With an exposure of only one second we obtained results showing that if we had been able to bend the plate to the required focal length, it would have given us a spectrum from wave length 30 to 60, giving us 1300 lines; the effect being very sharp lines in the neighborhood of the *H* and *K* calcium lines, and pictures of the chromosphere proper and the reversing layer showing 50 to 75 prominences. The difference between the calcium and the hydrogen lines was shown very prominently indeed. A plate out of focus showed the yellow sodium line, and the coronal lines at 1474 which were seen much stronger on the opposite side of the sun from that where the ordinary bright lines are seen. This green line was much brighter than on the western side of the sun. There is no line which seems to be evenly distributed on the two sides of the sun. The green line seems to be gathered into a very short arc on the western limb. On another plate taken 25 seconds later, the exposure was 5 or 6 seconds, and on that these coronal lines are shown all the way round. There are four lines not obtained before, the substance producing them seeming to be about the same as that giving the green lines. They show the strongest at some distance from the sun's surface, say three to five thousand miles above the solar surface, although they are visible close to the solar surface. Another interesting thing was the beautiful way in which the carbon band showed with an exposure of one second. It was very strong a hundred

miles from the solar surface. One word with reference to future work: I think it is very desirable to use a concave grating in the ordinary way, but instead of using a dense lens of quartz, to use a very large concave reflector of a focal length of 100 to 150 inches, which would give an image of the sun an inch and a half in diameter; and then we could use a wide open slit so as to be certain to get the limb of the sun inside of it. At Pinehurst we had to cut down the slit in consequence of the curvature, and had an image of the sun only a third of an inch in diameter to work with. In future work much the best plan will be to use two concave mirrors, one of a short focus for the dense lines, and the other of a considerable focal length, the slit being placed between them. In this way we can let the light come through the narrow slit, and then the larger and long focus mirror will act as a condenser, and there will be no disturbance whatever in the focus. A telescope of high power should be used, and focused very carefully; and the light reflected from the second mirror would give us parallel light; and if the adjustments were well made the image would be very sharp. By that method we ought to be able to obtain very satisfactory results.

Winslow Upton. I will report briefly the work we did near Norfolk by very ordinary astronomical and photographic processes. We had a four-inch telescope for visual work; and we had an equatorial stand upon which a camera was carried by clockwork. The camera contained two very long focus lenses. We also had four ordinary cameras, which we used for the purpose of obtaining the extension of the corona. We found that the duration of the eclipse was shorter than calculated. By the English and the American Ephemeris it was 101 seconds, while we made it 99 seconds. In the visual observations we used a magnifying power of 50 or 60. At the time I did not notice the hooded arrangement of the corona, this gigantic hood overlapping the strata, shown in the photographs of the eclipse of 1878. In our observations to determine the place of the moon, and in the search for an intra-Mercurial planet, we carried out the instructions of Professor Pickering, which were to expose for 60 seconds. Our cameras covered a range about 25° along the sun's equator, and with 8 or 10 gigantic plates we covered that region; but we got nothing except one or two bright objects, possibly only the planet Mercury, and there seemed to be star trails instead of images, perhaps from a fourth magnitude star. I think the exposure might have been longer; for I feel confident that we could have exposed 80 or 90 seconds without fogging the plate. For the spectroscopic work we used triple-coated plates, especially made for us by the Seed Co., and also orthochromatic plates. With regard to the development of the plates we decided to vary from the instructions of the Committee. The instructions were that we should use an exceedingly weak developer and develop a long time. Professor Pickering on the other hand recommended a strong developer, and a great deal of bromide. As we had duplicate plates we decided to try both plans, and I think on the whole that we obtained better lines with the strong developer and the bromide restrainer. I have here some lantern slides which I will show.

Ormond Stone. The University of Virginia took part in the eclipse observations at Waynesborough, N. C. We used a 40-foot camera. I desire here to express my gratitude to Professor Campbell for the advice he gave us at the beginning, and for his kindness in assisting in the preparations, and also in developing our plates for us. Our preparations were begun very early and we went to the camp about a month before the eclipse took place. At the time of the eclipse the sky was perfectly clear,

and we took a very good series ef pictures, showing a vast amount of detail clear down to the sun. We have learned much of what ought to be done in the future. Eclipse problems are constantly changing and new methods are used. In the adjustment of the frame in position, I think it ought to be done by actual observation of the stars, rather than by experiment. We happened to get our carriage some inches out of place, but by our observations we brought it within 1–16th of an inch of its true place. We may study the photographs as to the forms of the filaments, and the relations of the chromosphere to the protuberances; but that ought to be supplemented by visual observations with the telescope. A six-inch telescope with a magnifying power of 50 will show us ten times as much as can be seen in the best photographs. But the observer sees it and no one else, so that we still need the photographs. The plates were exposed by Mr. Mayo, who had himself constructed the apparatus. Mr. Morgan had a four-inch equatorial, and I used a six-inch equatorial and observed visually. It was our purpose to examine each of the prominences in succession to see if we could perceive any distortion. We could see no relation to the protuberances, and our visual observations simply confirmed what was shown in the photographs. At the eclipse of 1878 the filaments were well seen, and each one stood out distinctly. At this eclipse there was not that sharpness, and there seemed to be a strong background of something else to the equatorial filaments. In 1878 at the poles the rays were very much as in this eclipse. In the equatorial region there was a perfect network of filaments. They started out and moved right back with an incurvature in the opposite direction, so that there were a number of filaments side by side. They are certainly not hyperbolic curves, but we do not know what they are. One suggestion is that they are meteoric or comet streams. Our exposures were mostly short; we had nothing exceeding 12 seconds. It would seem well to have some longer exposures. I am glad the Naval Observatory has secured a plate with a longer exposure, on which we can see longer filaments and learn more of the nature of the curve. By tracing the curve back to the sun's surface we may be able to get some idea of its form. We had a number of small cameras mounted on a polar axis and managed by Mr. Lyon, and with these we used a color screen such that the green line was not seen, but on an open space. I think in another eclipse we ought to get more color screen pictures with our large cameras.

Mr. C. C. Abbott presented a report of observations at Waynesborough.

Mr. Dorsey explained the mode of determining the polarization of the bright coronal lines.

The Secretary read the following reports from Edwin A. Frost and E. E. Barnard upon their observations of the eclipse made at Wadesboro, N. C.

E. B. Frost. Four spectroscopes were employed: 1. A small plane grating in front of a lens of 2-inch aperture and 20-inch focus, for visual use in determining proper instant for exposure for obtaining 'flash' spectra.

2. Train of 3 large flint prisms, at minimum deviation for λ4230, in front of photographic lens of 3½-inch aperture and 42-inch focus; for photographing flashes and cusps and coronal spectra.

3. Small concave grating used directly, without slit; focus 30-inch for photographing flash spectra.

4. Large plane grating of 20,000 lines to inch, in front of visual lens of 3½-inch aperture, 42-inch focus, with red screen, for photographing red end of spectrum of cusps, flash and corona. All placed in horizontal beam 13-inch diameter reflected from

cœlostat. Time called by Professor A. S. Flint; exposure of 2 and 3 operated by Dr. G. S. Isham of Chicago; signal for exposure given by visual observation of flash with 1 by E. B. F., who operated 4.

Results: 1. The flash spectrum was visually observed in the yellow near D_3 at second and third contacts. The number of lines seen, however, was not as large as might have been expected; some 12 or 15 were seen in the small field employed. These were broken by the prominences, and were doubtless chiefly lines of the usual chromospheric spectrum. No attempts at measurements were made, as the object of the visual observations was to find the instant for making the exposures for flash spectra. The dark lines of the cusp spectrum were sharp and distinct 45 seconds before the computed time of totality. A glance into the eyepiece at the middle of totality showed no coronal rings in the field, which did not include the region of 1474 K.

2. Objective train of three large prisms (mean path of the rays in the glass was 8 inches). Five exposures were made, on two plates, all of which were successful.

(*a*) The two spectra of the cusps, taken before and after totality, show many bright (chromospheric) lines in addition to the dark lines. The region covered by the plate was necessarily limited, by the construction of the plate holder, and includes only from λ 4380 to λ 4025. Fifty bright lines were measured on the second cusp spectrum.

(*b*) The first flash spectrum shows no continuous spectrum, but will furnish the wave-length of many bright lines. Something over 100 lines appear to be measurable within the above limits.

The second flash spectrum was overlaid by a band of solar spectrum, the third contact occurring some seven seconds earlier than was expected and indicated by the count. This plate, however, shows a large number of bright lines, 275 having been measured on it by the writer between λ 4380 and λ 4027. Young's list of chromospheric lines contains 27 in that region.

(*c*) The coronal exposure was of 30 seconds, but could have been longer to advantage. The rings of $H\gamma$ and $H\delta$ (chromospheric) are strongly impressed, showing the prominences, and next in intensity is the strong chromospheric rings of wave-length λ 4078. Four other rings are seen in whole or in part, the coronal ring at λ 4231 being next in intensity, although faint. It shows none of the broken appearance which characterizes the chromospheric rings. Another ring of longer wave-length than the $H\gamma$ may also prove to be coronal. The continuous spectrum of the corona is quite strong. This plate has not yet been accurately measured.

3. The small concave grating, without slit, was used for first and second flash.

On the first some 70 lines (bright) are shown between $H\beta$ and $H\delta$; on the second about 110 in that region, with about 50 others less sharply in focus, chiefly on the violet side of $H\delta$.

4. With the limited time for exposure, the sensitiveness of the plates, of the 'Erythro' brand was insufficient to record the flash spectra. The plate exposed for 60^s to the corona shows no impression, as was indeed expected. The spectrum of the cusp ten seconds after totality shows some dark lines and a few bright lines, and the second flash spectrum is seen in traces.

E. E. Barnard. The following preliminary statement concerns the photographs of the corona obtained by the Yerkes Observatory expedition to observe the total eclipse of the sun at Wadesboro', North Carolina, May 28, 1900.

The weather conditions were as near perfect as possible.

The most important instrument was a 61½-foot horizontal cœlostat with an excellent 6-inch photographic objective made

specially for the purpose by Brashear. The light was thrown into the telescope by a very perfect plane silvered mirror made by Mr. Ritchey for the instrument. The equatorial mounting that carried this mirror was also made under Mr. Ritchey's direction though not for this special purpose. It was, hower, adapted by him for a 48-hour revolution. This instrument was driven by the clock from Professor Hale's 12-inch equatorial. The mounting also carried a 15-inch mirror for Professor Frost's spectroscopic work.

A long horizontal tube connected the 6-inch objective with the photographic house. This tube consisted of a wooden frame work covered with red water proof paper and was whitewashed on the upper part. A white cloth covering was also placed over it with a few inches of air space between the cloth and the tube. Frequent diaphragms were put along the inside of the tube to prevent stray light or reflections.

The photographic house in which the plates were to be exposed, was about 30 feet by 6 feet; it was of wood made thoroughly light proof by heavy red paper.

The plates were placed in a carrier 15 feet long, which ran on ball bearings in a light-tight sheath—the exposures being made through an aperture in the sheath where the image fell.

With this carrier, by rapidly shoving it along, one plate after another could be substituted for exposure with the minimum loss of time, about four seconds for a change. With it seven plates were exposed. Three of these were 14x17 inches, and the other four 25x30 inches. The following exposures were given these plates.

No.	Exposure.	Size of plate.	Kind of plate.
1	$\frac{1}{2}^s$	14 x 17	Cramer double-coated.
2	2	14 x 17	Cramer 'crown.'
3	8	25 x 30	" "
4	30	25 x 30	" "
5	14	25 x 30	" "
6	4	25 x 30	" "
7	1	14 x 17	Cramer double-coated.

A stop, regulated by hand, fixed the position of the carrier at each exposure, so that the image should always fall central on the plate. The plates were all set parallel to the celestial equator.

The exposures were made by a wooden exposing shutter, with a round aperture on one end of it. This shutter was controlled by two cords, one from each end, running into the photographic house. These cords being held taut, one in each hand, the exposures were made with absolute certainty and rapidity, by a quick pull with one or the other hand.

In the actual work, the carrier containing the plates was moved forwards by Mr. Ritchey after each exposure. At his signal 'ready' the proper exposure was given by me, the time being counted from a sounder beating seconds.

The seven plates were exposed without a hitch, the program being successfully carried out.

The signal for the beginning of totality was given by Mr. Putnam of the U. S. Coast Survey, who observed the contacts with a telescope at the Smithsonian Station, a hundred feet or so distant.

The exposures were begun immediately upon the signal for totality. A signal was also to be given ten seconds before the close of totality. This last signal was heard while preparing for the last exposure, and certainly within three or four seconds of this the exposure was made; the sun was then just appearing as a small speck of light—the end of totality having apparently come sooner than was expected.

At each exposure the image was seen on the plate and I was at once impressed with the striking likeness it bore to the corona of January 1, 1889. Though the moon appeared very black on the plates, the corona itself was disappointingly feeble. In the casual inspection, during exposure, the prominences were not noticed and but little detail was visible.

On the night before the eclipse each plate had been carefully backed with a non-halation solution consisting of caramel and burnt sienna. The plates were at once placed in the carrier ready for exposure.

In preparing for the eclipse the question of sensitiveness of the plates to be used was carefully discussed with Professor Hale and we finally decided to use the most sensitive plates. The result justifies our decision and shows that with a long focus instrument like this ($\frac{1}{128}$) the plates cannot be too sensitive.

Two of the plates were Cramer double-coated and were supposed not to be so sensitive as the Cramer 'Crown.' Plate No. 1 (double-coated) was purposely left unbacked. No. 7 was also double-coated and backed.

Up to this writing, plates No. 2, 5, 6 and 7 have been developed. They are remarkably sharp and perfectly defined, and show the prominences and inner corona very beautifully. The polar fans come out magnificently. There is a great deal of beautiful detail in the inner corona that promises to richly repay careful study.

Great care is being exercised in the development of the plates to insure not only detail near the sun but to get also as great extension as possible.

Other instruments used were a 6-inch and a $3\frac{1}{2}$-inch portrait lens, mounted on the equatorial mounting formerly owned by Mr. Burnham and kindly loaned by Professor Comstock.

A $4\frac{1}{2}$-inch Brashear portrait lens, and a very sharp focus (1:1.9). Voightlander lens of 4 inches aperture, were strapped to a fixed post and were used for quick exposures on the corona. Three Sonader lenses, kindly loaned by Mr. Ellerman, were placed on a small heliostat mounting and were given an exposure extending nearly through totality, with three different kinds of plates.

The plates with the small instrument have, in part, been developed. The results show an extension of the corona between 3 and 4 diameters of the moon. The corona, as shown on these plates, is singularly like that of January 1, 1889. There is a broad fish-tailed extension spreading out to the west more than half way to the planet Mercury, which shows on all the plates. A long, thick set, more or less pointed mass, extends to the east for several diameters of the moon. The poles are surmounted with fan-like systems of rays—the whole entirely characteristic of the sunspot minimum corona.

The results with the cœlostat show that it is by far the simplest and best instrument for securing photographs of the corona on a large scale. The results also show that the images could be made very much longer with much gain. An instrument of this kind for photographing the corona could safely be made several hundred feet in length with a corresponding increase of aperture.

G. A. Hill. A few remarks with regard to the observations especially made to determine the position of the moon. In several of the reports the idea is presented that the time predicted from the ephemeris was not accurate. This I think was caused by the position of the observer and his station not being exactly known. At Lawrenceville we had a transit telescope, and we computed the times of contact. The times of the second and third contact agreed within 0.5 second with the ephemeris, and the times of the first and last contact came within 1.9 second. The duration of the totality was exactly given from the ephemeris. At Barnesville we had a clock giving seconds, with the 60th second left out, and the gentleman who took the time there lost his count, omitting that second, so that we were a second late in our time. But this was not the fault of the ephemeris, but of the man who counted on the clock. I was much gratified with the condition of the

sun on the morning of the eclipse. The sun was unusually steady, and we succeeded in obtaining contacts that were quite satisfactory.

M. B. Snyder. I would like to call attention to the prominence on the southwest corner of one of the plates. I observed it visually, and was puzzled by the peculiar extension of that prominence. There was a decided change in its character. At the top there was a gradual fading of the prominence and the corona; and this is brought out by the photographs shown.

Ormond Stone. In the observation of contacts it seems to me that there is a different way from the usual way, and which is preferable to it. The method used by myself in the eclipse of 1878 was used by Mr. Morgan at the present eclipse, at my station. I am not aware that it has been used elsewhere. It gets rid of the difficulty which arises from the fact that, especially at the first contact, we recognize the contact by an indentation. In other words, we do not observe the first contact at all, but we recognize as the first contact a time which is not the first contact. The method which we used was this: A divided scale was placed in the field of the telescope, with 20 divisions on each side, with every fifth division a little longer. This was placed at the proper angle to correspond with the diurnal motion, and carefully focused. When the indentation in the sun's edge had the length of one division a record was made; when it had reached the length of two divisions another record was made and so on through. By examining the report of the eclipse of 1878 you will see how closely the different results agreed. The observations of the present eclipse have not been reduced, but it seems to me that some such method as I have described is far preferable to the method of noting the time when the indentation is large enough to be fairly visible.

Edgar Frisby. The observation of the first contact involves the knowledge of about the position where the contact will occur. Of course it is always a little late, but when the observer knows the position and is looking very closely, the error must be very small. We should expect the last contact to be very close; but I have found, from the experience of three or four observers, that the agreement was not so close as in the first contact. It seems after all to be more difficult to note the exact time of the last contact than of the first.

A Member. With reference to the remarks of Professor Upton, I may say that I had charge of the camera, attached to which was a visual telescope so that we should be able to know just when to get the flashes. The time of the first flash was successful, and I made the exposure and got a number of bright lines. The second exposure was made after about ten seconds, during the totality, and it partially showed the bright lines. The chromospheric rings were not well shown because we had no driving clock. The attempt to get the flash at the last contact was not successful in consequence of the shortening of the time of exposure. After this I made an exposure of 10 seconds every minute, and several plates showed a continuous spectrum. One minute after the third contact the lines were beautifully shown. The cause of the apparent continuous spectrum was perhaps the fact that the background was not sufficiently bright to show dark lines. Possibly they were all there but not thoroughly brought out.

M. B. Snyder. You will remember that a year ago I called attention to the use of the phonograph as a means of recording the time. During this eclipse I have actually tried the phonograph, with results quite satisfactory. We concluded that we might observe the shadow bands, and erecting a screen directly opposite to the sun we ob-

served these bands and recorded the results by the phonograph. In this I was assisted by Mr. Thompson.

Mr. Thompson. At Mr. Snyder's request I will say that having gone to observe the eclipse, I undertook in co-operation with him to watch for the shadow bands. I stood about 20 feet away, and about three minutes before the sun disappeared I noticed very faint streaks. I did not communicate this to Mr. Snyder until they became unmistakable. At that time they were moving, so far as could be determined, at about the rate of a man running, say, 20 or 30 feet per second or perhaps faster than that. The points were curved, and they appeared to be broken into little curves 4 or 5 inches apart. I should think the dark places increased in width as totality approached until the darkness seemed to cover the entire space, and a few seconds before the totality everything had gone into confusion, and there was no definite progress after that. Just after the totality I noticed that the same direction as before was maintained, but that there was another system of bands moving in the opposite direction. Thus there were two sets of bands moving in opposite directions, for a very short time, of which Mr. Snyder has the record in the phonograph. Those that moved in the original direction persisted, fading out gradually in about the same time they took to come on before totality. I think it very likely that the confusion which I noticed just before totality was due to the action of the two systems of bands.

Brown Ayers. As I have never figured in the scientific world as an astronomer, it may have been presumptuous for me to undertake to do so much as I did in the observation of the eclipse. But I realized that my position at New Orleans was near the center of the totality, and there were parties who came to our place to observe the eclipse, but who located on other grounds. The location seemed to be poor, and one or two who thought of coming gave it up. But I thought as we were the only institution of any magnitude on the line of the eclipse it would be absurd if we should allow it to pass without any observation whatever. When we received the little ephemeris sent out by the United States Naval Observatory, we laid our plans as to what we could do with the appliances at hand. The result was that I tried to observe the shadow bands, and to get the contacts as well as possible. That was my program until about ten days before the time of the eclipse; when Chancellor Fullerton of the University of Mississippi, wrote and offered to loan me a fine telescope of 15-inch visual and 9-inch photographic aperture, made by Grubb. I received it gladly, and mounting it prepared to take some photographs with as much accuracy as I could. But the main point I had in view was to get the times of contact. There were different points in the city carefully determined and marked by the Coast Survey, so that I was able to get my position accurately, and I could get Washington time by signals in my observatory. I was able to make a very large number of comparisons to obtain the local time. The plan I adopted was a combination of visual and photographic work. We took the pictures as rapidly as we could feed the plates into the telescope, the position being carefully adjusted by star-trails, and we had a snap shutter automatically recording the time at which each picture was taken. This gave us a large number of photographs of which the time was accurately determined. We were so fortunate as to get a plate showing the Bailly beads, and the next plate was blank; so that we have secured a very accurate observation of the time of the second contact. At the time of the third contact I was peculiarly fortunate; for just as the plate was exposed by the snap shot, there was a

sudden flash, and I think I have the time of that contact photographed pretty closely. In addition to my work, Professor Fullerton, who was within a few feet of my observatory, made visual observations; and I think we can work up some successful results. Instead of the collection of astronomers they had at Waynesborough, we enjoyed isolation, and the eclipse was an old story with us before they could do anything in Georgia or North Carolina. There was one little thing I should like to note, to which my attention was called by Mr. Brashear. It is that one large prominence which on my plates is distinctly turned over, half an hour later in Georgia was standing straight up. This shows the change which had taken place after my observations at New Orleans. We had beautiful weather until the eclipse was over, when it immediately clouded up. We took over 50 photographs; and although we had no astronomers with us, unless we count Professor Fullerton as one, I think we did our best.

Winslow Upton presented a report of shadow bands observed by John Ritchie Jr. of Boston, and also those observed by Edwin F. Sawyer.

W. J. Humphreys. Mr. Ayers states that just as the exposure closed, at the second contact, he looked up and saw the last of the sunlight go; that the totality had not quite begun when this exposure was made. The plates shows not only no dark line spectrum at all, but there were bright lines. This seems to show a very remarkable phenomenon. At the second and third contacts there was a slight depth of solar photosphere and a great deal of halation; but immediately after the third contact there is no halation whatever, and the lines are extremely bright, showing the crescent due to the plate and the chromosphere. I noticed something that seemed to be of the same character. It was $2\frac{1}{2}$ to 3 minutes before the second contact. I was observing with a telescope made up of a field glass and a small reflector, using no slit, and when I began observing I saw the narrow crescent of the sun as a dark crescent in the spectroscope. The form of the lines, instead of being straight, was crescent. Then the bright lines began to encroach upon the dark crescent, and the dark crescent began to shorten and broke up into a number of short crescents, and these came down to a narrow line, not a dark line but a bright line, which continued for nearly a second and then suddenly disappeared, when the whole field was filled with bright lines. The *F* line, at the time of totality, extended around almost the complete circle, perhaps three-quarters of the way round, and at that time there were only a few other lines observable. The magnesium lines showed somewhat. One thing I wish to call attention to is that the photograph shows that at the second exposure there were only bright lines and no dark lines at all. Mr. Gilbert was under the impression that the photosphere had not quite gone. The base of the chromosphere was intensely bright, and this is required for the reversal of the small lines which are produced very close to the photosphere. I also call attention to the fact that where the spectrum was almost continuous there was a very decided difference between the brightness of the bright lines and the dark lines. Very many of the dark lines were reversed, and there was no such phenomenon connected with the bright lines.

W. W. Campbell. This layer which is spoken of is shown in one photograph which was taken before the totality was ended, absolutely that and nothing else, and it extends over 150° on the western side of the sun. Another point that I wish to emphasize is our appreciation of the kindness of the Weather Bureau in sending us bulletins. The Weather Bureau obser-

vations showed that a strip of territory in western Georgia and in eastern Alabama, had the best chance of securing observations of the eclipse. But in fact I think we had the poorest weather. A minute after the eclipse was over the sun was thickly clouded over, a large mass of clouds coming up from the west. In connection with this I will say that a member of our party made it a part of his business to observe the corona, and he found that he could visually observe the corona 70 seconds after the eclipse was over. We thought it was not worth while to take photographs at that time, but we could probably have followed the corona longer except for the clouds which came up and covered the sky at that time. With regard to the green ring in 1898, which was shown very much broken up, indicating great coronal activity there, in this eclipse we had the green material piled up in large masses.

A Member. I was watching the region of *F*, to learn how to keep the image in the slit. It was not a gradual nor a sudden change, but a flickering one; and it went out a number of seconds after the totality in exactly the same manner that it came in, so far as its brightness was concerned. I am sure that if the best arrangements were made to economize the light, a grating of $21\frac{1}{2}$ inches focal length could be successfully used. The light from that part of the sun which was upon the slit was not more than 1/200th part of that thrown by the cœlostat upon the ruled portion of the grating. If we could in some way economize the slit we might get good results from it.

Mr. Jewell. Probably the flickering spoken of was due to a slight tremulousness of the atmosphere. An interesting thing to observe was the carbon bands, which were exceedingly bright at a distance of 100 or 200 miles above the sun's surface, and which extended out about 400 miles.

OBSERVATIONS OF EROS AT THE COMING OPPOSITION.

Simon Newcomb. Some one has divided astronomers into two classes, those who talk about things to be done, and those who go to work and do them. In the present case I am afraid we shall have to enroll ourselves in the first class, because it is not easy to do anything in this matter, the situation in this country not being favorable to the determination which we have in view. You are doubtless all aware of the great interest attaching to this remarkable asteroid. It may be said to supply us with what we have long been wanting, an object admitting of exact observation, which at proper intervals will come so near the earth that the solar parallax can be determined with greater precision than by any other method. It would hardly be possible to get one more exactly to fill the bill. The minimum distance of Eros from the earth is 0.15 of the distance of the earth from the sun. It follows, therefore, that at certain times it will be about as near to us as observations can advantageously be made. Were it to come very much nearer the additional advantage would be slight, for the reason that the elements of its orbit could hardly be known with sufficient accuracy to give us greater advantage; 0.15 is all that we can ask for, since that distance will diminish the effect of errors of observation by six or seven times. We have made very little progress in this direction for fifty years, and may now hope for something more. Yet in Eros, the Fates or whoever rules our destiny, have supplied us with something very tantalizing. It turns out that the nearest approach of Eros to the earth occurs only on rare occasions. A nearest approach occurred in 1894, and another approach as near as that will not occur until after the middle of the coming century. But an approach as near as can occur in the next twenty years will take

place in the coming autumn, when its minimum distance from the earth will be about 0.3, or twice its absolutely shortest distance. It is very desirable that astronomers who devote their attention to the determination of the solar parallax, should take advantage of this opportunity for we shall have a parallactic displacement of Eros exceeding three times and possibly five times the solar parallax. But there are many drawbacks, the planet is faint; it will probably be, when nearest to us, but little above the ninth magnitude, and it will therefore be scarcely possible from its rapid motion to photograph it, or to ascer- its true position upon the plate at any given time. It is difficult to ascertain just how the observations upon it should be made. With the view of getting the ideas of those who are interested in the matter, and of ascertaining how the stations can best be planned, I have published in the *Astronomical Journal* a paper upon this subject, accompanied by diagrams, to aid in the selection of the observing stations. These diagrams give four projections showing the position of the earth as seen from the direction of Eros, at different periods during the opposition. (A copy of one of the diagrams was placed upon the blackboard.) If we imagine an observer looking down upon the earth from Eros, the relative parallax between any two points upon the earth's surface, will be the distance of those two points as seen by the observer upon the planet. These diagrams therefore show us the advantages and disadvantages of any two stations we may choose. The line of sunset, showing when Eros will be visible to us, will be nearly the same for several nights in succession. Taking for example December 16th; in that part of the earth upon the left of the diagram, it will be day, and observers there cannot see the planet at all. It is only in the remaining region that an observer can see the planet. I will mark upon this portion degrees of latitude, showing the position of different observatories. On the circle of 60° north latitude, Pulkowa and Helsingfors are situated, their position upon the circle depending upon the time of observation. It is evident from the diagram that the parallax at those points will be very great; and those two places have the advantage of being near one end of the base line giving the greatest observable amount of parallax. The asteroid being so far north on December 16th, does not set at all at those places. It is evident that the best corresponding point, giving the longest base line, will be in the portion of the diagram south of the equator. The planet at that time is only visible about 15° south of the equator and the only observatory in that region is at Arequipa. If then observations are made at the Arequipa observatory, for an hour or two, and if observations are also made at Pulkowa or Helsingfors for an hour or two, it will give us the longest possible base line for these observations. Observations may be made at other points, but they will be less available in the determination of the parallax than those made at Pulkowa or Helsingfors and at Arequipa. Another point for consideration is that if the two sets of observations are made at different times, the planet will be moving in the meanwhile. Its motion is very rapid, and an element of uncertainty is introduced by the necessity of determining the amount of this motion. If the observations at Pulkowa or at Helsingfors and at Arequipa are not simultaneous this uncertainty affects the result. The question arises therefore whether simultaneous observations can be obtained at the ends of this base line. To determine that question we have only to imagine a map of the world upon this diagram. As the observer at Eros comes in sight of the oceans and continents of the earth, passing before him, he will see the two stations, at Helsing-

fors and at Arequipa; and observations cannot be simultaneous unless he can see both at once. We see that the conditions are extremely favorable. While Arequipa is passing along in *this* region, near the sunset line, Pulkowa will be passing along in *this* region, near the opposite horizon; so that we may combine observations made at Arequipa early in the evening, near the end of twilight, with those at Pulkowa several hours later in local time, in the latter part of the night, and they will be nearly if not quite simultaneous. We can thus obtain the maximum parallax, which will be 48″, with a little allowance for motion, from the observations of those two stations. Later in the season the Cape of Good Hope will appear above the horizon, and then another combination can be made, the circumstances will be less favorable. We might have observations at Paris, Greenwich, Potsdam, etc., but they will afford less favorable means of comparison than Pulkowa and Arequipa, which give the longest possible base line. As to our own position, we can observe in connection with Arequipa, but not easily; and in connection with the Cape of Good Hope, but when Eros rises with us it will set at the Cape of Good Hope. On the whole we cannot too strongly impress upon Professor Pickering the desirability of getting at Arequipa the best observations that can be made there, for combination with all the other stations. As to the method of making the observations, undoubtedly we must depend upon photography. There is no heliometer at Arequipa, and at the Cape it is not certain that it is available. It will not do to depend upon the heliometer. We must depend upon photography, and it is not easy for one not an expert to make valuable suggestions with regard to that. An additional complication arises from the rapid motion of the planet. It does not appear certain that the planet can be photographed at all so as to be available for this purpose. It must rest in one position long enough to take a picture or there will be no trace of it upon the plate. Professor Pickering says that there will be no difficulty in getting a trail; but that will not answer the purpose. In view of the fact that most of the stars will be brighter than the asteroid, photographs may be taken by focusing upon the planet and taking a trail of the brighter stars. But the difficulty remains of getting the time from the trails. Another difficulty is that we have no first-class photographic apparatus in the equatorial region, available for this purpose. I conclude that the combination of Pulkowa and Helsingfors with Arequipa ought to be our main reliance for obtaining the value of the solar parallax.

G. A. Peters. I should think that owing to the fact that the planet is upon the horizon during the observations for parallax, the light would be much diminished, and that the photographic plan would in that respect suffer a disadvantage in comparison with observations obtained visually. Unless large telescopes are used, and the atmosphere is extremely clear, there is danger that no observations can be obtained in that way.

Professor Newcomb. It is fortunate that at the stations where the planet is near the horizon the heavens are remarkably clear. At Arequipa, Pulkowa and Helsingfors the vapors near the horizon are but a minute fraction of those in the clime of Washington. It is a curious meteorological fact that here the vapor is generally so dense that good seeing near the horizon is much more rare than at the stations selected.

C. C. Abbott. Would it not be well to use isochromatic plates?

Professor Newcomb. That is a detail that we shall probably have to leave to those who take the photographs.

S. J. Brown. I have given this matter

some attention. It seems to me that micrometric observations could be carried on simultaneously, especially in the United States. We have Pulkowa, Strassburg, Charlottesville, Evanston, Madison, Princeton, Washington, and the Lick Observatory, and the Yerkes, where work can be advantageously done with large telescopes. While the factor of the parallax is very much smaller in the case of simultaneous micrometric observations, the main thing seems to be to secure the necessary co-operation, and to prepare a careful plan beforehand. The motion of Eros is so large that the observations will have to be very nearly simultaneous. The difference in the factor for parallax between Pulkowa and Washington is about 1.1. It is not much greater between Pulkowa and the Lick Observatory. It is 1.92 at Charlottesville; 1.26 for the Lick Observatory, 1.13 for Washington, and about the same for Princeton; so that by simultaneous observations between Pulkowa, Strassburg, Greenwich, and the observatories in the United States, we may obtain a parallax of about 15″ between the 1st of October, and January 16th. My impression is that stars should be selected not more than 60″ from the planet, and that they should be compared at nearly the same Greenwich mean time at all the stations. In this way observations may be obtained extremely valuable for determining the parallax. I am not certain but that the planet can be photographed to advantage: but it would require a long time, and the motion of the planet would be uncertain; so that the advantage I think would be outweighed by the accuracy with which micrometric observations could be made; and if referred to the same stars, eliminating the motion of the planet, we might expect precision. The position of the planet can be ascertained closely enough, and its parallax known so nearly that we can measure it to advantage in telescopes varying from 16 to 40 inches in aperture in the United States. I had started to draw up a letter to be sent to the directors of the observatories of Pulkowa, Strassburg, and Greenwich in Europe, and in this country at Charlottesville, Princeton, Madison, the Yerkes and the Lick, asking that co-operative work might be carried on, and asking for a selection of the stars. If we assume 60″ for the greatest distance from the planet, and a parallax varying from 15″ to 30″, this will give a distance from 60″ to 90″; and you can go down to the 10th magnitude, the magnitude of the planet, and find in the field a number of suitable stars, which should be symmetrically situated in rectangular co-ordinates. In this way it seems to me that we can obtain from these large telescopes observations of as great accuracy as those obtained by the photograph, although the distance may be twice as great and the factor twice as great. I do not see that anything practical can be done by this association. It must remain for some one to take the initiative; and there ought to be no difficulty in getting suggestions to perfect such a simple plan, such as may result in saving the day for the observations of Eros. Photographic observations would have to be confined largely to Pulkowa, Berlin, or Potsdam, and Arequipa, the Cape of Good Hope will not come in until along towards the end of the opposition. It seems to me therefore that micrometric observation, under these circumstances will be a matter of considerable importance.

Professor Newcomb. We want the stars as nearly of the same magnitude as possible. In a space 60′ square, the mean distance of the stars would be about 6′. The planet moves about 2″ of arc in a minute. An observer making a careful micrometric measure must take at least ten or twenty seconds to make up his mind about the bisection. During 10 seconds the planet will have moved over a space of $\frac{1}{3}$″ of arc, a quantity not appre-

ciable but still large enough to leave open the question whereabouts during that period of 10 or 20 seconds occupied in making the bisection, the planet actually was at the time assumed for the bisection. There arises an uncertainty which may be systematic. It may be different for different observers and for different places. This uncertainty seems to preclude dependence upon the process of micrometric measurement. In the case of the satellites of Saturn we know that results are obtained differing enormously.

S. J. Brown. And for the satellite of Neptune the difference is still greater.

Professor Newcomb. There are some systematic differences attending micrometric observations, but these can be diminished if we take our observations to avoid them. We have the method of difference of right ascension and declination, when we take the right ascension entirely free from the declination, taking the right ascension at the time of passing a certain thread. If the star is chosen of about the same magnitude as the planet, it will be free from one source of systematic error, but there may be a great many others. If each set requires a minute or two, and if two threads are used, you may make 60 comparisons in an hour, which I think will be more than can be done with the micrometer. There is then much room for systematic error.

G. W. Hough. I think the most accurate work can be done with the micrometer. With an 18½-inch telescope there is no trouble in seeing an 11th or 12th magnitude star; and it is only necessary to get the time of the bisection and the position angle and distance. The time can be known within a few seconds, and ten observations can be made as readily as one. So we can get a final result free from systematic error.

Simon Newcomb. That may be quite possible for a 12th magnitude star.

G. W. Hough. The method of differences of right ascension and declination I use very often; but then we must depend upon the stability of the telescope. It may require 1½ to 2 minutes to pass from the star to the planet, and there is a liability to error from the action of the wind or from other causes. In photographic work the star will be near the horizon at the time required for its observation, and the time required will be very long, especially if we use isometric or other such plates; and that will be a great objection.

W. W. Campbell. It seems to me that both the methods suggested should be submitted to actual trial before the entire campaign is based upon either method. It ought to be possible by means of known asteroids moving rapidly to obtain photographs, by allowing the stars to trail or the asteroid to trail, and we could learn with what accuracy the position of the asteroid can be obtained upon a certain plate. Furthermore it will be easy to test the method of observing faint stars near the asteroid. In large telescopes it will be possible either to get a star of the 12th or 13th magnitude within 1′ of arc, and by selecting stars in different directions from the asteroid it will be possible to eliminate practically all the personal element entering into the question. In my experience in micrometric measures there is never an uncertainty greater than 2 or 3 seconds as to the exact instant of bisection, and the motion of the planet in that time, for that particular observation, will be small as compared with the accidental error probably made. If we take ten observations together the motion of the planet will be practically unimportant. My main point is that the method fixed upon should be actually submitted to test before the campaign is started. It would be a serious matter if the campaign should be carried on, and afterwards it should be ascertained that in consequence of some oversight the results are of very little importance.

G. C. Comstock. If 12th magnitude stars are to be used in the comparisons, can provision be made to secure the observation of the same stars at the different stations?

W. W. Campbell. A photographic plate can be secured from which the accurate positions of these stars can be ascertained.

G. C. Comstock. Is there time for this before the campaign begins?

W. W. Campbell. I think there should be a carefully prepared plan.

S. J. Brown. In relation to the observation of differences of right ascension and declination, all the observations will have to be made before we can determine the corrections, because the changes of right ascension and declination will not be in proportion to the elapsed time in consequence of the rapid motion of the asteroid. We need to eliminate the systematic errors. You mark your time, and your measurement does not correspond with the time you have marked. That leads to a systematic error. If we can eliminate this I think that observations of great accuracy can be made. In this case it is proposed to select stars which shall not be more than 30 seconds in time away from the planet. In micrometric measures the suggestion is 60″, which added to the displacement of the planet amounts to 90″. Above that distance there would be a rapid increase of probable errors. The main difficulty appears to be in the selection of stars down to the 11th or 12th magnitude.

W. W. Campbell. It seems to me that the method of observation of transits is objectionable. With the equatorial telescope there is a tremendous personal equation in the observation of stars and comets. Undoubtedly there will be a similar personal equation in the observation by transits of an asteroid and a faint comparison star differing 3 or 4 magnitudes from the asteroid. There are many things that may occur; the refraction may be changed, or the position of the instrument may be changed if we wait a minute or two for the comparison star to come along. Our experience in the observation of comets shows that very much better observations can be secured if we throw overboard the method of observing differences of right ascension and declination.

G. W. Hough. I disagree as to the time required for the observations, and I have made thousands of observations of differences of right ascension and declination. But one measure of the position angle is better than anything you can get from differences of right ascension and declination.

S. J. Brown. We have to shift the micrometer 90° in taking the position angle. Suppose the planet to move 1″ per minute, the change in the angle is not proportional to the time, and the change in the distance is not proportional to the time. I have come to the conclusion that differences of right ascension and declination can be determined with greater accuracy than differences of position angle and distance. One point we wish to agree upon is a uniform method of measurement. Observations cannot well be compared if one observation is made upon one plan and another observation upon a different plan. I have no doubt that the difference in magnitude is a very important factor in determining the position. In my observations of Titan I reduced my observations by the method of least squares, and all the equations gave positive residuals except one; and that could only be explained upon the theory of a variable personal equation from difference of magnitude. Even with faint stars it would be difficult to use differences of right ascension observed with the chronograph or eye and ear. I think we ought to have the experience of those who have had experience with stars of the magnitude to be used as to their method of treating them. The photographic method I think is subject to

special dangers on account of the low altitude of the asteroid in the southern hemisphere.

W. W. Campbell. The base line which Professor Newcomb has indicated has a great extent, but as to successful working much depends upon a wise compromise which ought to be considered. At the expense of longer observation the observers are going to work near the horizon, where the refraction will be more uncertain. Furthermore the image to be measured will not in all cases be satisfactory on account of the vapors rising near the horizon. What amount of difference in the base line will outweigh the differences in the atmosphere we shall have to compete with? It appears to me that a base line which gives only two-thirds of the parallax may be more accurate than one which gives five-sixths, in consequence of the better conditions under which the observations may be made.

G. C. Comstock. We should not be misled by the drawing upon the blackboard, because it is not drawn to scale. Taking the indicated positions, we shall not bring our observations very near the horizon. I agree that we should not seek the maximum possible base line, there are other considerations in the problem, but we should get the longest base line we can without encroaching upon the horizon, and Pulkowa and Arequipa do not crowd the observations upon the horizon.

S. J. Brown. We have to take the telescopes as they can be found. At Pulkowa they have an excellent telescope, and at Arequipa also; and they are of identical power. These two stations are fixed, therefore, by the facts of the case. And so the Cape of Good Hope will be fixed as another station. The photographic program is fixed. I think the matter will be settled according to the circumstances as they exist now. We have three photographic telescopes of 13-inch aperture distributed very favorably in the southern hemisphere, and three or four in the northern hemisphere. The micrometric problem is one which I think the astronomers of America can agree to co-operate in, and I think we ought to have considerable influence in inducing Strassburg and Greenwich to co-operate with us. If we can secure the agreement of astronomers in this country and send word to those in Europe before October 1st, we may possibly save the situation, and perhaps produce results comparable in accuracy with those from the photographic plates. The planet may be observed at Pulkowa and Washington from October 1st to January 16th, the observations at Pulkowa extending to an hour angle of 6 or 7 hours. Then the planet would have an altitude of 30°; and the factor can be extended from 15″ to 30″ or even 35″. But I have been unwilling to write to foreigners until I could ascertain the opinions of astronomers here.

Winslow Upton. I think if Professor Pickering were here he would assure us that Arequipa is remarkably well situated to adapt it for a southern station. The condition of the climate and the atmosphere is favorable, and it has an elevation of 8000 feet above the level of the sea, ensuring good definition. Again, the twilight there is always short. We can be perfectly sure that Professor Pickering will use the facilities of the situation to the utmost. But there is a difficulty from the unknown law of refraction. The discussion of the observations will take the differential form, but the law is not known at all. These considerations do not apply to Pulkowa, but there the planet will not be low.

F. L. Chase. With regard to diurnal motion, will it be possible to eliminate the motion of the planet?

S. J. Brown. I think the micrometric method in that respect out of the question. The motion of the planet is about 10′ in 10

hours, and it would be difficult in a large telescope to measure that arc of 10′. We find that with the great telescope at Washington we are limited to 4′ or 5′. Beyond that we do not get good results. You cannot make the observations without shifting the eyepiece; and I have made up my mind that for the purposes of micrometric observation the diurnal plan would be out of the question.

J. G. Hagen. Referring to the photographic method, there is a difficulty which has not been discussed. The motion of the planet, in taking a photograph, which may require an hour or more, will produce a trail. There is an opportunity to choose between allowing the planet to trail, and allowing the stars to trail. Perhaps the same object, of securing an accurate position for the planet upon the plate, can be attained by interruption of the exposure. The time of the motion can be known within ten seconds; and while the planet, of the 9th magnitude, may be obtained upon the plate, the bright stars may be obtained in a series of dots, each exposure giving an independent determination. Whether these interruptions should be made once in ten seconds or once in a minute is a question for experiment. I merely make the suggestion of a method that may remove the difficulty.

Adjourned.

GEO. C. COMSTOCK,
Secretary.

WASHBURN OBSERVATORY.

SCIENTIFIC BOOKS.

Curing and Fermentation of Cigar Leaf Tobacco. By OSCAR LOEW. Report No. 59, U. S. Department of Agriculture. 1899. Pp. 34.

Physiological Studies of Connecticut Leaf Tobacco. By OSCAR LOEW. Report No. 65, U. S. Department of Agriculture. 1900. Pp. 57.

The great and growing importance of the tobacco industry in the United States has led the Department of Agriculture to undertake an extensive series of investigations, covering the mapping of areas of soil suitable for raising tobacco, studies in fermentation, improvements in breeding and selection, the conditions of growth and manipulation in foreign countries and the question of supplying tobacco to foreign markets. Dr. Loew has been detailed to carry out the chemical part of these researches, and the above-mentioned documents set forth the results thus far obtained by him and by others, and contain an abundance of material, both of scientific interest and practical importance.

From the consumer's standpoint essential constituents of tobacco are: (1) nicotine; (2) certain compounds, the chemical nature of which is wholly unknown, which impart to the leaf its acceptable flavor or aroma, and which differ in quality and quantity in different grades of tobacco. It is upon the latter, rather than upon the nicotine, that the commercial value of the prepared leaf mainly depends. The amount of nicotine is largest in the fresh leaf and undergoes marked diminution during fermentation, while the aromata are developed during the processes to which the tobacco is subjected. There is no apparent relation between the color, aroma and amount of nicotine.

The preparation of tobacco comprises two stages, curing and fermentation or 'sweating,' sometimes supplemented by aging or 'cold sweating.' During the earlier part of the curing stage the cells are still alive, and the resulting changes are physiological, embracing among others the transformation of starch into sugar, and the partial respiratory consumption of the latter, or its transference to another part of the leaf and reconversion into starch. After the death of the cells, the proteolytic and oxidizing enzymes attack much of the protein, fat and tannin, and give rise to changes of color and flavor. The curing stage is followed by one of fermentation, which goes on under the influence of air and moisture, and which is accompanied by a marked rise of temperature. During this stage there is a notable decrease of nicotine, but an improvement of flavor and aroma. The fermentation was ascribed by Suchsland to the action of bacteria, and he attempted to impart to German tobacco the peculiar Havana flavor

by inoculation with pure cultures of bacteria obtained from the Cuban leaf, but without success. Perhaps the most interesting part of Dr. Loew's work is the proof that microbes play no essential part in the normal tobacco fermentation, and that the active agents are oxidizing enzymes. These exist in the green leaf, but are able to manifest their peculiar power of utilizing atmospheric oxygen also during curing and fermentation, when unopposed by the normal physiological processes of the living cells. At least three of these are present—an oxidase, a peroxidase and catalase. The documents contain much interesting information on the subject of oxidizing enzymes, together with speculative discussion of their nature and mode of action, which may or may not stand the test of future developments. With the revival of the study of catalytic phenomena now in progress from the standpoint of physical chemistry it is to be hoped that vegetable physiology will not have to wait long for important light on this still obscure subject.

The documents encourage the hope that the preparation of tobacco, which up to the present has been based on empirical procedure, will before long be conducted in as scientific a manner as is already the case with alcoholic beverages. They also afford an excellent illustration of the manner in which a government department, existing and working solely for practical purposes, is nevertheless compelled to encourage studies of broad scientific interest.

H. N. Stokes.

The Birds of Eastern North America. Part II, Land Birds. Key to the Families and Species. By Charles B. Cory. Illustrated. Special edition printed for the Field Columbian Museum, Chicago. Boston. 1899. Small 4to. Pp. 131–387.

Ornithologists, during the rapid growth of popular bird study in the past few years, have witnessed the production of all kinds of bird literature. In the great variety that has been put forth, the general effort has been for untechnical descriptions with sufficient accuracy to stand the test of practical utility. Mr. Cory has accomplished this end to a considerable degree in several of his books. In the 'Land Birds,' we have neither an exhaustive manual nor a pocket key, but an easy ornithology for beginners in the shape of a profusely illustrated key. It is continuous in pagination with the volume already published on the water birds and the two are obviously intended to be used together, for the useful introductory preface and glossary of the first part are not reprinted in the second. The book begins with a key to families, illustrated by outlined drawings of bills, wings, tails, and feet, and much reduced halftones of species characteristic of the various families. After this comes the key to species, which is the body of the book; then follows a systematic list of both land and water birds, giving in general terms the geographic distribution of each. The species are divided into groups by absolute characters, such as length of wing and distinctive colors, which could not be misconstrued even by the veriest amateur; technical terms are avoided as far as possible. The descriptions are brief, scarcely more than diagnoses, but more detailed than those of an ordinary key. The illustrations are not of uniform excellence, but serve their purpose fairly well. They are conveniently inserted in the text which refers to them and are repeated when necessary. The book is a little large for use in the field, but for the actual work of identification at the study table it should be a most valuable aid, particularly to the beginner.

W. H. Osgood.

SCIENTIFIC JOURNALS AND ARTICLES.

The Journal of the Boston Society of Medical Sciences for June brings the fourth volume to a close and is accompanied by the index. The opening article on 'Pathological Changes affecting the Islands of Langerhans of the Pancreas' is by Eugene L. Opie, and 'The Influence of Defibrination on the Secretion of the Kidney' is discussed by Franz Pfaff and Vejux-Tyrode. 'A New Method of distinguishing Human from other Mammalian Blood in Medico-Legal Cases' is described by Ernest L. Walker, based upon the different characters of the granules of the polymorphonuclear leucocytes of the blood of various mammals and the ability to recognize these differences in dried blood by appropriate treatment and methods of staining. 'Some

Laboratory Apparatus' is noticed by Frederic P. Gorham and 'Methods of staining Flagella,' by W. H. Robey, Jr. Finally an abstract is given of a paper on the 'Action of Drinking-water on Metals,' by Charles Harrington, in which among other interesting points it is noted that the same water may act very differently at different times.

The Osprey for June begins with Part VI. of 'Birds of the Road,' by Paul Bartsch; some 'Bird Notes,' by Lady Broome, are reprinted from the *Cornhill Magazine*, and in the fourth part of William Swainson and his Times,' by Theodore Gill, we are told of his literary work. The editor discusses the 'Significance and Etymology of the Word Mammals,' calling attention to the fact that the commonly accepted derivation of the word is incorrect, and that the term Mammalia, from which it comes, was coined by Linnæus in analogy with *animalia*, to describe the class mammalia which he was the first to correctly define.

The Auk for July is an unusually large number, containing articles of much interest, not only to the professional ornithologist, but to the general reader. The 'Occurrence of *Larus glaucescens* and other American Birds in Hawaii' is noted by H. W. Henshaw, and this is followed by a graphic and gracefully written article, 'Notes on the Breeding Habits of the American Golden-eye Duck or Whistler,' by William Brewster. The 'Ecology of the Maryland Yellow-throat and its Relatives' is treated at length by William Palmer. 'Notes on a Few Species of Idaho and Washington Birds' are contributed by John O. Snyder, and a 'Description of a New Shearwater from the Hawaiian Islands' is given by H. W. Henshaw. A. W. Anthony tells of the Nesting Habits of the Pacific Coast Species of the Genus *Puffinus*.' E. W. Nelson gives 'Descriptions of Thirty New North American Birds,' and Reginald Heber Howe describes 'A New Subspecies of the Genus *Hylocichla*.' 'An account of the Nesting Habits of Franklin's Rosy Gull (*Larus franklinii*), as observed at Heron Lake in Southern Minnesota,' is given by Thomas S. Roberts, and the concluding paper 'Notes on a Collection of Bahama Birds,' is by Outram Bangs. The articles on nesting habits are all illustrated, the plate showing the burrows of the Wedge-tailed Petrels being remarkably interesting. The customary numerous notes and reviews complete the number.

THE *New York Medical Journal*, long published by Messrs. D. Appleton & Co., has been sold to Mr. R. Elliott, an advertising agent in New York City. It is to be hoped that the Journal will remain under the same editorial management as at present, as it is one of the few American medical journals that have consistently maintained a high standard.

The Proceedings of the Zoological Society of London for June contains Dr. A. Smith Woodward's full paper on the much-discussed remains of *Grypotherium* (*Neomylodon*) *listai* from Patagonia. The specimens are described in detail and the affinities of the animal made clear. They were found on the floor of a cave, in a large enclosure, associated with cut hay and much excrement, and Dr. Woodward concludes that we can hardly refuse to believe that this great ground sloth was actually kept and fed by an early race of man.

DISCUSSION AND CORRESPONDENCE.

KITE *vs.* BALLOON.

EDITOR OF SCIENCE: The high kite-flight described in SCIENCE of July 6, 1900, was exceeded on July 19th, when, by means of six kites attached at intervals to four and three-quarters miles of steel wire, the meteorograph was 'lifted 15,170 feet above Blue Hill, or 15,800 feet above the neighboring ocean. At the time that the temperature was 78° near the ground, it was about 30° at the highest point reached, the air being very dry and the wind blowing from the northwest with a velocity of 26 miles an hour.

The altitude reached in this flight probably exceeds the greatest height at which meteorological observations have been made with a balloon in America. The highest observations that have been published were made by the late Professor Hazen of the Weather Bureau in an ascent from St. Louis, June 17, 1887, to a height of 15,400 feet.

A. LAWRENCE ROTCH.
BLUE HILL METEOROLOGICAL OBSERVATORY.

CALLOSITIES ON HORSES' LEGS.

To the Editor of Science: Your inquirer concerning the callosities on horses' legs might gain an indirect suggestion from Ernest Seton Thompson's 'Wild Animals I Have Known' or—still more probably—from the same author's recent articles in the *Century Magazine* on the National Zoological Park. The suggestion that these callosities are vestigial organs for the secretion of specific perfumes gains some indirect but interesting support from the use made of their material by Rarey and other professional 'horse-tamers.'

W J McGee.

NOTES ON INORGANIC CHEMISTRY.

In a study of the radiation of uranium, Becquerel finds that these rays are deviated in a magnetic field. When the uranium compounds are treated with barium salts and the barium then precipitated as a sulfate, the radio-activity of the uranium is decreased, but Becquerel has not been able in this way to obtain a uranium salt which does not show some activity.

A somewhat similar series of experiments is described by Béla von Lengyel in the *Berichte*. The barium sulfate obtained from the uranium mixture was found to be strongly radio-active, as well as the barium carbonate and chlorid derived from this sulfate. The author considers this synthesis of radio-active barium renders the existence of radium and polonium as elements exceedingly doubtful. This reminds one of the views of Le Bon that radium and polonium are merely allotropic states of barium and bismuth, corresponding somewhat to the inactive and the phosphorescent calcium sulfids.

Accounts have been published from time to time in the *Comptes Rendus* by Paul Sabatier and J. B. Senderens on the addition of hydrogen to acetylene under the influence of reduced metals in a finely divided condition. The action of copper, iron and cobalt have been most recently described, the union taking place below 200°. With copper, ethane, ethylene, and other hydrocarbons are formed, and if hydrogen is in excess no acetylene is unacted upon. With iron in addition to ethane and ethylene, benzene and higher unsaturated hydrocarbons are produced. Cobalt is found to give a much larger yield of ethane than nickel.

The direct preparation of a number of binary compounds of aluminum is described by Henri Fonzes-Diacon in the *Comptes Rendus*. The sulfid, selenid, phosphid, arsenid, and stibid are all formed by the ignition of a mixture of fine aluminum powder with the element in question. In the case of sulfur and selenium, a little burning magnesium is necessary to ignite the mixture; with antimony, sodium peroxid serves the same purpose. When these compounds are treated with water, the hydrogen compound is evolved in a very pure state. In the case of phosphid the yield of the non-inflammable gas PH_3 is practically theoretical, and the same is true of arsin. The yield of stibin is less good.

A recent *Bulletin* of the College of Agriculture of Tokyo, contains a paper by Dr. U. Suzuki on the possible replacement of calcium in plants by strontium and barium. From an abstract in *Nature* it appears that both strontium and barium salts are poisonous to plants, though the addition of lime salts lessens the poisonous action to some extent. This is apparently very different from the action on higher animals, where, though barium is strongly toxic, strontium has little if any toxic action.

From *Nature* we note also a short article by C. E. Stromeyer from the *Memoirs* of the Manchester Literary and Philosophical Society, on the Formation of Minerals in Granite. He concludes that the temperature of granite formation need not be limited, nor need the interior of the earth be assumed as solid. The mineral composition of granites depends not only upon temperature and rate of cooling but also upon pressure. "Where the solid rock resting on the molten material is of low specific gravity and a bad conductor of heat the depth at which granite rock would commence to solidify would not be great, and most probably the quartz would crystallize first, forming, say, quartz-porphyry." In somewhat opposite conditions, at greater depths and pressure the quartz would remain fluid longer, forming feldspar-porphyry. Every intermediate condition is also conceivable.

THE curious observation is made by P. Villard in the last *Comptes Rendus*, that at the temperature of 1000° fused silica, SiO_2, is permeable to hydrogen.

THE abstract of a paper read before the Chemical Society (London) by John Wade on the constitution of hydrogen cyanid, is given in the last number of the *Proceedings*. From reactions with alkyl iodids and sulfates, it has appeared as if potassium cyanid had the constitution KCN while that of silver cyanid is AgNC. Wade now shows that when potassium cyanid is heated with alkyl potassium sulfate at a lower temperature, the isomeric isocyanid is often the principal product. He further finds that practically all the isocyanids can be converted into cyanids (nitrils) by heat. Since the formation of nitrils in the above interaction is thus accounted for, one of the chief arguments for the nitrilic constitution, HCN, of hydrogen cyanid disappears, and it seems possible that HNC represents the constitution of the acid, and that all the cyanids have an analogous constitution.

J. L. H.

MEDICAL EXHIBITS AT PARIS.

THE Paris correspondent of the *British Medical Journal* gives the following account of some of the medical exhibits at the Paris Exposition:

In the Pavillon des Armées de Terre et de Mer, at the end nearest the Pont de l'Alma, we first enter the Salon Pasteur. On either side of the entrance are cabinets filled with cultures of different microbes. In the center of the room is the bust of Pasteur on a pedestal, round the base of which is an octagonal case containing a retrospective exhibition of the work of Pasteur. Here we see the manuscript of the thesis presented before the Faculty of Science in 1847 on molecular dissymmetry; the microscope used by Pasteur to measure the angles of crystals, and models of various crystals; his work on fermentation, with the original apparatus used for the study of butyric acid fermentation, and the apparatus for the study of living anaërobic microbes. Pasteur's researches on spontaneous generation are illustrated by the apparatus to prove that calcined air contained no germs, and the flasks used in the experiments on the organized dusts in the atmosphere, and opened by him on October 3, 1860, at the summit of Mount Poupet. In connection with his investigation into the diseases of wines and beer, flasks for the pure culture of yeast and experiments on the aging of wines are shown. The microscope used by Pasteur in his investigation of the diseases of silkworms is shown, together with baskets for rearing silkworms, chains of cocoons, and pigeon-holes for rearing isolated worms. Methods of sterilization are illustrated by the first model of the Chamberland autoclave used in Pasteur's laboratory, and by Chamberland filters. Virulent diseases are illustrated by the flask of putrified blood from which Pasteur obtained the anaërobic microbe which he called the 'vibrion septique.' U-shaped tubes from Pasteur's laboratory containing anthrax blood, with samples of the first and second vaccines against anthrax as supplied to veterinary surgeons are exhibited. Down to January 1, 1900, in France alone, 4,971,494 sheep and 708,980 cattle have been inoculated. Some manuscript notes by Pasteur on the experiments in his laboratory in 1881 on hydrophobia are shown, as are also his platinum spatula instruments for trephining rabbits and to remove the spinal cord, dried cords, etc.

On the right-hand of the Salon Pasteur is a model of the Pasteur Institute, with the recently completed Annexe of Biological Chemistry and hospital for hydrophobia and diphtheria patients. An adjacent glass case contains a bouillon culture of the bacillus of diphtheria in a large flat-bottomed flask, the trocar of Roux and Nocard with rubber tube to collect the blood from the immune horse, the jar in which the clot and serum separate; a small filter by L. Martin for experiments on the toxin, the large filter used to filter the cultures of diphtheria to prepare the toxin, the filtered culture, and bottles of the serum in liquid and dried form.

On the left-hand side is the exhibit of the Pasteur Institute at Lille, showing cultures of the plague bacillus of Yersin-Kitasato and the antiplague serum; venomous snakes, with Calmette's serum against snake bite; the sterilization of water by ozone, with numerous maps, plans and photographs of the Lille Institute.

In small rooms leading out of the Salon Pasteur is a portion of the hygiene exhibit of foreign nations, the major portion being in the Champ de Mars. Germany here shows a large model of the Imperial buildings for sanitary administration, a map of the mineral waters of Germany, numerous plans and elevations of the many sanatoria for the open-air treatment of consumption; also numerous graphic models to show the increase in the population, the fall in death-rates, the hospital accommodation, etc., the most striking being black and red cubes showing the number of deaths from small-pox in 1862–76, that is, 199,410, compared with the number of deaths in 1882–96, that is, 3291; in 1897 there were only five deaths from small-pox. Vaccination and re-vaccination became compulsory by law in 1775.

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*SIGMA XI, THE AMERICAN ASSOCIATION AND THE GEOLOGICAL SOCIETY OF AMERICA.**

An unsigned article in Science (June 22d) entitled 'Sigma Xi at the American Association for the Advancement of Science,' calls attention approvingly to a movement to associate meetings of this Greek-letter society with those of Association. The rapid rise of the Sigma Xi in American universities is cited, and it is affirmed that "as an honor society it promises to take a leading part in our universities in which science holds a prominent place." It is urged that "it has become a representative honor society for the ablest students of science in the institutions where it is established." Respecting its intent, the following authoritative quotation is made: "In establishing a new chapter * * * in each case we should make sure that we entrust the power of distributing the honor of membership only to such persons and institutions as are capable of giving the education and training necessary to the carrying on of scientific investigation."

It is scarcely necessary to make these quotations to show that the fundamental feature of the Society is the promotion of a class distinction based on academic preparation. However laudable this may be in itself considered, it would seem to be inharmonious with the fundamental purpose of the Association, which is the development and dissemination of science among all people without regard to race, age, sex or previous conditions of intellectual servitude.

From professional relations the writer should not be inappreciative of the value of university training and of academic achievement. Nevertheless, it seems to him that the purposes of the Association are unqualifiedly democratic and that the spirit of science is equally so, and that therefore the only distinctions which the Association should foster or sanction, if it fosters or sanctions distinctions at all, are those which are based solely upon scientific productiveness. And this productiveness should be honored quite irrespective of its connection with the fortunate conditions of academic appointments and opportunities, or with the adverse or even hostile conditions under which much good science has been developed. The movement therefore to connect the Sigma Xi with those of the American Association seems incongruous.

As set forth in another article in the same number of Science, some fifteen special scientific societies have already become correlated with the Association and have much increased the complexity of the proceedings. This movement seems to be an inevitable consequence of the differentiation of scientific work, and is scarcely less than necessary to the continued success of the Association, but it has already brought some inevitable conflict of interests and not a little congestion of programs and appointments. Between these and the increased number of social functions, it has already come to pass that there is little time left for that personal conference and that informal sociability whose basis is 'shop talk,' which formed so large a factor in the attractiveness of the earlier meetings of the Association. If now in addition to these laudable complications, the attention of a considerable number of the members of the Association is to be diverted in the interest of an academic honor society and a precedent established for the meeting of other societies whose basis is not strictly congenial to that of the Association,

* Editorial article from the *American Journal of Geology*.

it is not clear where the limit of congestion will be found.

Between the lines of the article referred to, the imagination is tempted to read a hint of a desire for that rank and dominance in the Association which the members of Sigma Xi attained in university circles, and it is not unnatural to anticipate that the fraternity might unconsciously play a part in Association politics not unlike that for which Greek-letter societies are famous throughout the university world. To those who pride themselves upon rank and band themselves together because of rank it is not unnatural that official expressions of rank should be sought through the unconscious influence of fraternization.

It is not altogether foreign to the subject of this discussion to note the increasing encroachments of formal social functions upon the meetings of the Association and not less perhaps upon the meetings of the Geological Society of America. Without doubt a certain measure of formal contact with general society is helpful to the ends sought by the Association. At the same time it must be recognized that formal social functions are largely the province of the leisure class and that from the very nature of the case they remain so, for leisure and the means of leisure are prerequisite to their effective cultivation. Equally from the nature of the case, the devotees of science do not usually belong to the leisure class because real success in science involves strenuous endeavor and an almost unlimited devotion of time. The diversion of time to social functions during the meetings of the Association should, therefore, be zealously watched and restrained within limits which are compatible with the efficient conduct of the primary purposes of the Association. Particularly is this true of the Geological Society which has no organic relation to general society. The movement in the direction of social formality has already crowded hard upon the point where the first requisite preparation for a meeting of the Association or of the Geological Society is the packing of a dress suit, and the second is the preparation of an after-dinner speech, preparations that are none too congenial to the great mass of hard workers in science.

SCIENTIFIC NOTES AND NEWS.

A BUST in bronze of M. de Lacaze-Duthiers was presented to him on July 1st by representatives of the University of Barcelona, consisting of the rector M. de Luanco and Professors Lozana, Mundi, Arazona, Lopez-Sancho and de Odan Buen. M. Gréard, vice-rector of the university of Paris, M. Frederic of Liège, M. Delage of the university of Paris and M. Leygues minister of public instruction, made addresses to which M. de Lacaze-Duthiers replied.

DR. J. HOWARD GORE, professor of mathematics and geodesy at Columbian University, Washington, has been appointed by President Loubet, juror-in-chief of the Court of Appeal of the Paris Exposition. He is already juror-in chief of the International Congresses for the United States.

ON the occasion of the celebration of its centenary, the University of New Brunswick, at Frederickton, conferred an honorary degree on Dr. J. G. Adami, professor of pathology in McGill University.

DR. BENJAMIN IDE WHEELER, of the University of California, has returned from his visit to the East, during which he was given the LL.D. degree by Harvard and Brown universities.

MR. JOHN C. MERRIAM is at present in the fossil fields of eastern Oregon, where he has charge of an expedition making paleontological collections for the University of California.

DR. HERMANN TRIEPEL has been appointed prosector at the Anatomical Institute at Greifswald.

THE death is announced of Mr. Georges Marye, curator of the museum at Algiers.

THE Rev. Thomas D. Weems, of Decatur, Ill., has given his archæological collection, numbering eleven hundred and forty specimens, to the Powell Museum of the Illinois Wesleyan University. The collection contains figures, vases, pictured stones, ceremonial stones, tablets, pipes, arrowpoints, spearpoints, celts, sinkers, knives, saws, hammers, scrapers, plummets, discoidals, mortars, pestles and copper, bone and shell implements and ornaments.

The Rev. Dr. John A. Kumler, of Springfield, Ill., has offered to provide for the cases to contain the above collection.

A Society for School Hygiene has been established in Germany and will hold its first meeting at Aix la Chapelle, in connection with the meeting of German Men of Science and Physicians beginning on September 17th.

The position of physicist in the Geological Survey will be filled by a civil service examination on August 21st and 22d. The salary of the office is $1800 per annum. The subject of examinations and the weights are as follows:

1. English (essay)	8
2. German	8
3. French	8
4. Laboratory training	28
5. Mathematics	20
6. General physics	28
Total	100

A telegram was received on July 24th at the Harvard College Observatory from Professor W. R. Brooks at Geneva, N. Y., stating that a bright comet was discovered by him July 23d at 13^h (premsumably Eastern time), in R. A. $2^h\ 43^m\ 40^s$ and Dec. $+ 12°\ 30'$. Motion northerly. Stellar nucleus and tail. A subsequent letter from Professor Brooks states that he found the position of his new comet July 26, $13^h\ 25^m$ Eastern time ($18^h\ 25^m$ Greenwich Mean Time) to be in R. A. $2^h\ 46^m\ 30^s$ and Dec. $+ 21°\ 1'$.

The Conference Scientia, an informal society organized at Paris for social purposes, held its sixth meeting on June 28th, the occasion being a banquet in honor of M. Darboux, the eminent mathematician. An address was made by M. Ch. Richet to which M. Darboux replied.

More than 300 papers have already been offered for the meeting of German Men of Science and Physicians which takes place at Aix la Chapelle beginning on September 17th. General addresses will be made by Professor Van't Hoff, Berlin, on the 'Development of Chemistry'; by Professor Oscar Hertwig, Berlin, on the 'Development of Biology'; by Professor Naunyn, Strasburg, on 'Internal Medicine including Bacteriology and Hygiene,' and by Professor Chiari, Prague, on 'Pathological Anatomy and External Medicine.'

At a recent meeting of the Museums Association at Canterbury, Professor W. M. Flinders Petrie advocated his plan of building large but inexpensive sheds to house ethnological and archæological material. The plan is to acquire about a square mile of land within an hour's ride of London and to begin to build large galleries uniformly on what might be called a gridiron plan. The proposed galleries should be about 54 feet wide and 400 feet apart, so that after completion there would be room for additions six times as large on the intermediate ground. The plan would involve the construction of about 400 feet of gallery per year, or eight miles in the century, leaving six times the space to be covered by irregular additions as required. The financial demands of the scheme for land, building and staff might be met by a fixed charge of £10,000 per year.

The report of Commissioner of Patents Duell for the fiscal year ended June 30th last shows a total of 25,540 patents granted, including reissues and designs. The receipts of the office were $1,358,228, and a surplus of $110,402 was turned into the treasury.

For the past two months an installation of wireless telegraphy has been in operation at the lighthouse at Borkum and the light ship 20 miles away. Communication is established with the North German Lloyd Steamship *Kaiser Wilhelm der Grosse* in its voyage into and out of Bremenhaven and other shipping news is transmitted. This is the only wireless telegraphy installation transmitting ordinary intelligence on a commerical footing.

The Chancellor of the German Empire has issued an ordinance to the effect that the Réaumur thermometer will not be admitted to official control after January 1, 1901. This will lead to the exclusive use of the centigrade thermometer in Germany.

Lieutenant Peary's relief steamer *Windward*, has entered the harbor at Port au Basques, at the southwest extremity of the island, with part of her machinery disabled. Pieces to replace the broken sections have arrived, but it will probably require a few days to make the necessary repairs. The delay may

seriously disarrange the ship's plans for reaching the far north.

THERE is again a new universal language to rival Volapük and Spokil, and which calls itself Bolak, in English the 'blue language' or 'the language colour of heavens.' We quote from the English circular issued from Paris. This language appears to be the joint production of Léon Bollack and Raoul de la Grasserie. The following extract is from the same circular:

"To give to all the possibility of receiving news from whole the world and of understanding them *without any translator.*

"To give to all the possibility of crossing whole the world and of making themselves understood *without any interpreter.*

"And a more generous IDEAL may be reached, owing to the coming of an INTERNATIONAL LANGUAGE which, letting remain the *native idiom* of each one, would become the unique FOREIGN LANGUAGE for ALL.

"It is indeed obvious to understand that this *facility of comprehension* between persons of different nationalities will forcibly raise a Holy Communion of thoughts and of pacification among peoples dealing together."

But we of the English tongue naturally ask why not adopt English as the universal language, since it is already more universal than any language has ever been in the history of the world?

THE daily papers report that Indians hunting on the east coast of Hudson Bay, north of Fort George, in the early spring, have a story that may eventually give the history of the fate of Andrée and his companions. Mr. George Renison, returning from Moose Factory, the Hudson Bay Company's post on the west coast of James Bay, says that the last packet from York Factory brought word that the Indians had found a quantity of wreckage, the bodies of two men and a man in the last stages of death. The Indians could not understand the language he spoke, but it was not English. He died while they were there, and they returned to their post without bringing away any evidence of the strange occurrence. As the Indians had never seen a balloon, the nature of the wreckage was judged only from descriptions given by them, but Mr. Renison says they described accurately a car and other fixtures that could belong to nothing else. Hudson Bay Company officers are firmly convinced that it is the Andrée party, and have sent out men, guided by the same Indians, to find and bring back evidence to establish the identity of the party.

A BRITISH Parliamentary paper has been published giving details of experiments performed on living animals in 1899. It appears from the abstract in the London *Times* that the total number of licenses in England and Scotland was 250, of whom 72 performed no experiments. Tables I. and II. give the names of all persons who held licenses during 1899. These tables afford evidence (1) that licenses and certificates have been granted and allowed only upon the recommendation of persons of high scientific standing; (2) that the licensees are persons who, by their training and education, are fitted to undertake experimental work and to profit by it; (3) that all experimental work has been conducted in suitable places. Table III. shows the number and the nature of the experiments performed by each licensee mentioned in Table I., specifying whether these experiments were done under the license alone or under any special certificate, so that the reader may judge which experiments (if any) were of a painful nature. Table III. is divided into two parts, A and B, for the purpose of separating experiments which are performed without anæsthetics from experiments in which anæsthetics are used. The only experiments performed without anæsthetics are inoculations, hypodermic injections, vaccinations, and similar proceedings, in which the pain inflicted is not greater than the prick of a needle. No experiments requiring anything of the nature of a surgical operation, or that would cause the infliction of an appreciable amount of pain, are allowed to be performed without an anæsthetic. The total number of experiments included in Table III. (A) is 1656. Of these there were performed under license alone 820, under certificate C 182, under certificate B 449, under certificate B + EE 205. In experiments performed under the license alone, or under certificate C, the animal suffers no pain, because it is kept under the influence of an anæsthetic from the beginning of the experiment

until it is killed. In experiments performed under certificate B (or EE or F linked with B) the animal is anæsthetized during the operation but is allowed to recover. The number of inoculations for the diagnosis of rabies performed in 1899 was 164, the steady decrease during recent years noticed in the report for 1898 having been maintained. Table III. (B) is devoted entirely to inoculations, hypodermic injections, and some few other proceedings performed without anæsthetics. It includes 6813 experiments. The total number of experiments (8469) is somewhat less than in 1898 (9151). The licensees were found in all cases to be desirous of acting in strict accordance with the spirit as well as the letter both of the Act and of the special conditions attached to their licenses. In Ireland nine licenses were in existence during 1899. Of these four expired, one was renewed, and two new licenses were granted. The experiments performed were 227 in number, 79 being under license alone and 148 under certificates. Two licensees performed no experiments. The animals experimented on were 171 rabbits, 43 dogs, 12 guinea-pigs, and one rat. The experiments appear to have been of a useful character, and either painless or painful only to a slight extent. The bulk of them were inoculations for the diagnosis of diseases, such as canine rabies and tuberculosis.

The Moscow Society of Physiologists, a branch of the Imperial Society of Friends of Natural Science, has, according to the *British Medical Journal*, undertaken to issue a journal entitled *Le Physiologiste Russe*, which is intended to make the work of members of the Society in physiology, physiological chemistry, histology, embryology, general pathology, and pharmacology known to that large section of the scientific world to which Russian is an unknown tongue. Original papers will be published in French or German; and summaries of all Russian work appearing elsewhere will be given in French. The first volume, which has already been issued, contains papers from the laboratories of Moscow, St. Petersburg, Warsaw and Tomsk. Among the writers are Professor Morokowitz, head of the Physiological Institute of Moscow, and editor of the journal; Professors Setschenow, Bogdanoff, Salaskine, Chalfeieff, and Kulagin. *La Physiologiste Russe* is sent gratuitously to all institutes of physiology, pathology, and pharmacology. As giving some idea of the relative numbers of such institutes in different countries, it is interesting to note that Austria receives 31 copies, Great Britain, 23, Germany 46, France 42, other European countries 59, the United States 29, South America 4, Asia 5, Australia 3, Africa 1. *Le Physiologiste Russe* is published and distributed with the help of contributions from friends of biological science, who have subscribed a capital sum of £2000 for the purpose.

UNIVERSITY AND EDUCATIONAL NEWS.

Lord Calthrope and his son have given to the University of Birmingham about 25 acres of land for its scientific department. The site is very suitable, permitting the establishment of the department nearer to the center of the city than would otherwise have been possible.

Mr. Ewan Richards Frazer, of Balliol College, is announced as the donor of £5000 for a pathological laboratory at Oxford.

Trinity College has received from Mr. J. M. Allen, of Hartford, a complete set of the Proceedings of the American Asssociation for the Advancement of Science.

The trustees of Iowa State College in their annual session made the following additions to the faculty of the college: H. J. Burt of the University of Illinois, assistant professor of civil engineering, this being a new chair; B. S. Lampear of Cornell University, assistant professor in electrical engineering; Lewis E. Young of Pennsylvania State College, instructor in mining engineering; I. A. Williams of Iowa State College, 1898, instructor in mining engineering.

The vacancy at Toronto University caused by the retirement of Professor Pike from the chair of chemistry has been filled by the appointment of Mr. William R. Lang, who has for some years filled the position of lecturer in organic chemistry at Glasgow University.

Professor Brieger has been appointed to the newly established chair of hydrotherapy at Berlin University.